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I.—Operative Surgery of the Eye.[1]

The invention of the ophthalmoscope by Helmholtz in 1851 has been of greater importance than was expected at the time. Not only did its introduction give an enormous stimulus to the study of ophthalmic medicine, but its use has since furnished many surprising revelations, and its powers have proved susceptible of immense development. By it a disease of the retina can be diagnosed with the same certainty as one of the cornea, the effects of intra-cranial as well as of intra-ocular pressure can be directly observed, and the refractive condition of the eye can be determined with accuracy, more in many cases than by the use of glasses.

The history of ophthalmic medicine naturally divides at the period of this invention. Progress and change had been comparatively slight since the time of Beer, except in so far as the views current in general pathology had been modified, and such modification had produced some alteration in treatment. The second period was inaugurated by the rise of the Berlin school, of which von Graefe was the head, and to whom it owes almost the whole of its fame. The first number of the 'Archiv für Ophthalmologie' appeared in 1854, and commenced with a paper by him, unsurpassed in ophthalmic literature. This was followed for the next sixteen years by a series of his communications

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[1] 1. *Traité des Opérations qui se pratiquent sur l'œil.* Par Édouard Meyer et A. de Montméja. Paris, 1871.

2. *Handbuch der gesammten Augenheilkunde.* B. iii ('Operationslehre,' von Prof. Arlt). Leipzig, 1874.

3. *Ophthalmic Surgery* (extracted from 'Erichsen's Surgery,' 6th ed., 1872). By J. F. Streatfeild.

4. *A Practical Treatise on the Diseases of the Eye.* By Haynes Walton. 3rd edition. London, 1875.

which, for originality and importance, will not be again approached. About the same time the anatomy of the eye was diligently studied, and chiefly by Bowman, and physiological optics were systematised by Helmholtz, and applied to practical purposes, especially by Donders. The contributions of minor importance, published in various countries, have become almost uncountable; there were, in the single year 1870, more than 600 original works, large or small, published on ophthalmic matters, the anatomy and physiology of the eye being included. The result of all this zealous study is that this department has advanced more rapidly than any other in medicine, and that it is now pre-eminent for fulness and accuracy of knowledge, and consequently for certain and successful treatment.

For some years the statements of von Graefe were widely received with distrust, too often in a hostile spirit. Nowhere was this more the case than in England, where the opposition culminated in an active controversy on iridectomy in glaucoma, and revealed a surprising amount of ignorance, rashness, and critical incapacity, which might be admired in a conservative politician, but was deplorable in surgeons supposed to be "of mark."

"When the novelty was fresh, our statistical notions of ophthalmic diseases were a little perturbated at hearing of the number of cases of glaucoma which were operated on. We thought the disease a rare one, in any of its forms, but especially in the chronic. On looking over the patients in general or special hospitals, in passing through workhouses, and inquiring into the pathology of inmates of blind asylums, or examining into the published tables from ophthalmic hospitals, we find the disease to be a rare one. Well, as we looked more narrowly into the records of the new cases, we found they were not cases of glaucoma at all—certainly not according to the standard laid down by the best authors, and accepted by the most practical teachers. The young gentlemen who were twaddling (if we may use the expression, and they will agree with us in its applicability ten years hence) on this subject were evidently but indifferently educated, we mean as oculists, and were writing about what they did not understand, employing a jargon of which they scarcely knew the meaning, and doing but indifferent justice to themselves, the subject, or the operators they were reporting."[1]

[1] "Medical Epidemics: Glaucoma and Iridectomy," a Review. From the 'Dublin Quarterly Journal of Medical Science,' August, 1860, p. 77. The feeling on the Continent was very different. See Testelin's letter ('Ophth. Review,' i, 168), written in March, 1864:—"La cause de l'iridectomie (appliquée à la cure du glaucome) ne compte pas d'adversaire sur le continent. Il est encore plus difficile de supposer que la discussion passionée et malveillante de ses antagonistes, que l'ignorance absolue (feinte ou réelle) qu'ils déploient, au sujet des decouvertes de l'oculistique moderne, ne contribuent pas à jeter sur la cause qu'ils défendent le plus profond discrédit."

It is astonishing what may be written by even able men. The review in ques-

More than ten years have passed since the publication of the passage just quoted, and a large amount of evidence has accumulated during the interval, yet no general work has appeared in which any attempt has been made to collect it together. It would be profitable to examine not only how iridectomy in glaucoma, but also how the many other operations introduced or re-introduced during the last twenty years, have stood the test of experience,—to ask what operations should now be admitted as established, and what rejected or left for future decision. A full, still more an exhaustive examination of these questions would be a long and difficult task. Space and time would preclude any but a hasty and superficial survey in this place of the whole of operative ophthalmic surgery, and such an outline would necessarily exclude any exposition of the reasons and experience by which recent innovations are supported. Hence we prefer to restrict ourselves to one portion of this subject, to the indications and results of the operations performed on the cornea and iris, or for diseases of these parts. Even then we shall be able to adduce but a little of the interesting and valuable matter at hand.

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tion contains many curiously erroneous statements, of which we may notice a few. Such are that Reichenbach first recommended iridectomy in 1767, and that modern iridectomists make "an aperture either in the cornea in front, or in the sclerotic behind the ciliary attachment of the iris." Reichenbach's essay has been twice reprinted (the reviewer characteristically gives no reference); the reader will find it in the 'Thesaurus Dissert.' . . . ed. E. Sandifort, iii, 437; 'Lugd. Bat.,' 1778; or in the 'Dissertationes Med. sel. Tubingenses' . . . ed. C. F. Reuss, iii, 278. Tubing., 1785. The object of the dissertation is to show that in cases of cataract with closed or contracted pupil, the opaque lens can be extracted, the pupil enlarging under its pressure, or being opened out by the needle. "Si igitur cohæsio pupillæ minus arcta mansit, tunc demum consilii est acu, quali ad depressionem utuntur, et qua ego ad apertionem capsulæ crystallinæ uti soleo, in extractione cataractæ, portam hanc aperire, et linea recta, seu potius oblique sursum vulnus in centro infligere, et ita portam hanc arte et marte aperire." The rest of the essay is correctly epitomised by Reuss, as "Extractio cataractæ hic occurrit; lens crystallina adhærens forcipe separata. Exemplum profert autor, in quo visus non redit, aliud extracti corpusculi per vascula adhærentis; rectum scalpellum præfert." Reichenbach thinks, indeed, that "si vero quis acum scindentem reformidet, non inepta inventio foret, instrumenti cilindracei subtilis extremitate excavati et in peripheria scindentis (quale lorarii seu frenarii ad foramina rotunda a loris exscindenda, seu quali chirurgi Galli ad cauteria infligenda utuntur, quo uvea pertundi, et formari possit, cujus experimentum facere data occasione forsan licebit." Such an operation would still be a novelty. The reviewer probably depended for his statement on Jüngken's reference, in his Operative Surgery of the Eye, to the last-quoted paragraph.

Again, we read in this essay:—"What benefit arose from all this? A very manifest one; the New Sydenham Society, in its last volume, published von Graefe's 'Three Memoirs on Iridectomy,' which form the subject of the present review. Was not that a benefit to the members of a society who, for neither love nor money, could procure a copy of that fine old work, John Woolhouse's Treatises on the Eye, or Bannister's book, and other essays on the diseases of the organ of sight which we might refer to!" John Woolhouse and Bannister!! What a selection!

A few words on the books quoted at the commencement of this article are necessary before we pass to our proper subject. The first on the list, by Messrs. Meyer and Montméja, is a handsome quarto volume, illustrated by numerous woodcuts, and twenty-two large photographs. The former are good; the latter are indifferent, and must add much to the cost. Each operation is carefully described; its indications and contra-indications are pointed out in the manner usual to systematic writers, but in a more than usually clear style. We find almost nothing to object to, and at the same time scarcely anything that can be fresh to those acquainted with recent ophthalmic literature. Take it all in all, we can recommend this work to any one in want of a perspicuous and reliable guide.

The second treatise is one of a series, principally by German writers, intended to ultimately form a complete system of ophthalmology. It is by Professor Arlt, one of the most learned and experienced ophthalmic surgeons in the world. It is particularly remarkable for the careful and minute description of each operation; every detail, every modification, is usually explained. We wish it had been still longer; a bibliography and index would have increased its value. As might be expected, it is wonderfully full of information; it will for years to come be indispensable for study and reference.

Mr. Streatfeild writes in a sketchy manner, is rather one-sided, and makes some statements which we think incorrect. His little pamphlet contains, however, many just and interesting remarks.

The first edition of Mr. Walton's book appeared in 1853, under the title of 'A Treatise on Operative Ophthalmic Surgery,' and was most favorably noticed by the late Mr. Mackenzie in this Review.[1] A large amount of valuable matter is to be found in its 1200 pages, whilst at the same time we think it deficient in some important particulars. Numerous excellent cases are related, and Mr. Walton constantly gives his own experience. Books of this class are never very abundant, and we are thankful to the author for this one. The practitioner will often have recourse to it with advantage. We must not forget to mention that the woodcuts are usually very good, and that the coloured drawings of diseases of the fundus, as seen in the inverted image, are some of the best yet published.[2]

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[1] 'British and For. Med.-Chir. Rev.,' xi, 433. London, 1853. See also the review of the 2nd ed. in vol. xxix, 319, 1862.

[2] The second figure on the second plate must be wrongly named, and the third figure on the same plate does not represent a typical case of retinitis pigmentosa. The first figure on the next plate represents retinitis albuminurica most successfully.

The *progress of operative ophthalmic surgery* has been much promoted by the use of anæsthetics, and by fixation of the eye and eyelids. Pain and pressure on the eye are objections to fixation, but the former can be annihilated by an anæsthetic, and the latter to a dangerous extent is usually the fault of the operator or his assistant. Many minor operations, such as paracentesis, the needle operation for cataract, and removal of foreign bodies from the cornea, can be and are continually performed without these auxiliaries, yet in unsteady or very young patients the necessary manipulations are much facilitated and rendered more exact by their use.

Peritomy (excision of the conjunctiva around the cornea). Obstinate pannus was often treated in antiquity and in the middle ages by destruction of the conjunctival vessels. Attention was called again to this treatment in 1862 by Furnari,[1] who advised the excision of the conjunctiva and of the subconjunctival tissues all round the cornea, so as to completely denude the sclera, and the free application of the nitrate of silver to the exposed portion. The cauterization has been generally abandoned owing to the risk. Hirschberg (Prof. A. v. Graefe's 'klin. Vorträge'. . . . herausg. v. J. Hirschberg, i, 232, Berl., 1871) says that, as performed by Furnari, the operation has destroyed no few eyes. Restricted to the removal of a small strip, without cauterization, it has been found useful by many operators. A. von Graefe recommended it in (1) trachomatous pannus after disappearance of the granulations, (2) in certain cases of the diffuse keratitis of scrofulous children, and (3) in the corneal infiltrations tending to sclerosis which accompany scleritis and sclerotico-choroiditis. "Peridectomy is an excellent means in these, and, indeed, in all sclerosing inflammations of the cornea."[2] Meyer has had very satisfactory results in cases of diffuse keratitis in adults, where all other means, employed for months, had failed.[3] Wells says[4] that in some cases of the very dangerous crescentic ulcer of the cornea "great advantage has been derived from syndectomy, either partial, if the ulcer was but of slight extent, or complete, if a considerable portion of the cornea had become involved." Horner has succeeded in arresting diffuse interstitial keratitis in some recent cases by its use.[5]

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[1] He had previously recommended, after the example of Scarpa, the removal of a strip of conjunctiva, a line broad, from the circumference of the cornea. (S. Furnari, 'Traité pratique des Maladies des Yeux,' p. 325. Paris, 1841.)

[2] See Brecht's papers on Corneal Peridectomy in the 'Deutsche Klinik,' 1871, p. 189, and the 'Archiv für Ophthalm.,' xx, 1, p. 113.

[3] l. c., p. 200.

[4] J. S. Wells, 'Treatise on Diseases of the Eye,' 3rd ed., p. 116. Lond., 1873.

[5] P. Jackowlewa, "Ueber keratitis interstitialis diffusa," Diss. Zürich, 1873, quoted by Prof. Saemisch in 'Handb. d. ges. Augenheilk.,' iv, 269. Leipz., 1875.

Median incision of the cornea was introduced in 1869 by Saemisch[1] as a means of treating certain spreading ulcers of the cornea, often with pus in the anterior chamber, to which he gave the name of ulcus corneæ serpens. The incision passes vertically through the whole thickness of the cornea, and should extend beyond the ulcer into sound tissue. The incision is reopened daily, so long as appears necessary. He states in his pamphlet that he had so treated thirty-five cases with only one failure. Nieden[2] reports eighty cases treated by division; of these fifty were accurately noted. The treatment failed in only two cases of extensive ulceration (4%). In forty-two cases, or 84 per cent., the progress of the ulceration was arrested by the first slitting, the same result being obtained in six cases or 12 per cent., by the second slitting; the ciliary neuralgia, which was more or less severe, ceased at the same time in all these cases.

"The ultimate result was in thirty-five cases (70 per cent.) opacities of the cornea, in eleven (22 per cent.), opacity with adhesion of the iris, and in two (4 per cent.) the patient did not return."

Other operators have employed a free incision in spreading suppuration and infiltration of the cornea. Mr. T. P. Teale has published a series of very interesting cases[3] to show "that suppurative affections of the cornea and iris, which do not rapidly yield to atropine and opiates, ought to be dealt with by direct incision through the median part of the cornea into the anterior chamber, just as much, as a matter of course, as one would incise a whitlow or a thecal abscess."

He noticed—

"That on incising in its early stage a circumscribed abscess in the layers of the cornea, a small white body sometimes escapes from the opaque spot, which, when placed under the microscope, proves to be broken-down corneal fibre, infiltrated with pus;"

in fact, a corneal slough, like the core out of a boil. He asks—

"May we not hope that the same treatment may prove to be of equal value in threatened sloughing of the cornea from purulent ophthalmia, in the exceptional cases of glaucoma which iridectomy fails to relieve, and in most acute affections of the eye which threaten the vitality of the cornea?"

Some forms of suppurative keratitis are probably due to infection. It has long been known that chronic inflammations of the lachrymal sac considerably diminish the prospect of success in extraction of cataract. Saemisch's remark that this

[1] T. Saemisch, 'Das Ulcus Corneæ Serpens.' Bonn, 1870.

[2] Nieden, "On the Treatment of Ulcus Corneæ Serpens," in the 'Archives of Ophthalmology and Otology,' iii, 1, p. 238. New York, 1873.

[3] 'Ophth. Hosp. Reports,' viii, 61. Lond., 1874.

relatively common disease renders even very slight injuries, which would otherwise have produced scarcely the least effect on the eye, most dangerous,[1] is worth remembrance in regard to the treatment of obstinate cases of lachrymal disease. Hirschberg,[2] in 80 per cent. of the cases of what he calls torpid infiltration of the cornea, found blennorrhœa of the lachrymal sac of old date, and remarks that "special stress is to be put on the epithet old; by no means rarely the patients in question are exceptionally stinking, with inveterate ozæna, Rhinitis ulcerosa." Saemisch found the same state of the lachrymal sac in 32 per cent. of his cases of ulcus corneæ serpens at Bonn. Recent experiments of Eberth, Leber, and Stromeyer have rendered it probable that the blennorrhœa is even more closely connected with the keratitis than had been supposed, and that possibly the process is due to infection with septic substances contained in the discharge from the sac. Partly as a consequence, further trials have been recently made with disinfectants directly applied to the eye (see the remarks by Horner, Schiess, and v. Welz, 'Klin. Monatsbl. für Augenh.,' xii, 432. Stuttg., 1874).

Staphyloma corneæ.—The old operation of excision is liable to be followed by considerable loss of vitreous, intra-ocular hæmorrhage, and suppuration; the result being a small and inefficient stump. For these reasons the wound has been sutured by Mr. Wilde, Mr. Critchett, and others, the threads being passed through the sclera, or through the conjunctiva and episcleral tissue (Knapp), or simply through the conjunctiva (Wecker). It has been objected to sutures passed through the sclera, that they "sometimes give much pain and keep up inflammatory symptoms during the protracted healing" (Streatfeild, l. c., p. 51). Even sympathetic disease of the other eye is said to have been excited by them (Knapp in 'Arch. für Ophth.,' xiv, 1, 273). v. Oettingen records a case in which the sutures caused an extremely painful panophthalmitis ('Jahresber. üb. d. Leist. u. Fortschr. im Gebiete d. Ophth.,' 1871, p. 231, from the 'Dorpater Med. Zeitschr.,' Bd. ii). According to Anagnostakis, excision of the projection with subsequent suture of the wound is mentioned by Celsus, Galen, Paulus Ægineta, and Aetius. Two needles were passed crosswise through the base of the staphyloma, which was then excised; the wound was closed by tightening the threads ('Annal. d'Ocul.' vol. lxviii, 1872). Abscission of nearly the anterior half of the eye has been preferred by some surgeons to excision of the staphyloma alone.

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[1] l. c., p. 9.
[2] 'Klin. Vortr.,' p. 210.

"The object of the operation is to remove a useless and painful or unsightly feature, and to gain, instead of it, a low stump as a good foundation for an artificial eye. If we are careful to remove the lens, which is liable to chalky and other degenerations, and the ciliary body, which is full of muscular tissue, nerves, and blood-vessels, together with the offending parts, we get rid of future sources of offence, and very much lessen the chances of irritation and inflammation in it, and of sympathetic irritation and inflammation in the other." (Streatfeild, l. c., p. 50.)

A greater improvement has been, however, the substitution of enucleation in certain cases. Arlt remarks that total, even partial, excision is dangerous, when the eye is amaurotic from increased intra-ocular pressure. Beer long ago opposed the operation in cases where there are symptoms of what he considered cirsophthalmia, which we now call secondary glaucoma. During, or at all events after, the operation, the veins at the outer side of the choroid give way; retina and choroid are forced forwards through the wound, with excruciating pain; excision of the parts prolapsed is usually required to stop the bleeding and relieve the suffering. Then comes panophthalmitis. In such cases it is not good practice to excise the staphyloma; the eye should be enucleated (l. c., p. 377).

Küchler preferred ('Neue operat. Heilmethode,' Braunschw., 1853, quoted by Arlt, p. 379) as a certain cure at any stage of total staphyloma, division of the projection, evacuation of the lens, and a frequent reopening of the wound. His directions have been strictly observed by Arlt, who has, however, abandoned this treatment, owing to the occurrence of panophthalmitis. For the same reason the latter surgeon does not approve of the passage of a thread through the base of the staphyloma.

"If the thread is removed too early, there is no shrinking; if it remains till there is greater reaction, especially till there is œdematous swelling of the conjunctiva bulbi and eyelids, the occurrence of panophthalmitis cannot always be prevented" (l. c., p. 379).

Passing over opacities of the cornea with the remark that Wecker has introduced a mode of colouring them with indian ink, and has found that both appearance and sight are greatly improved by the tattooing, we come to *Conical cornea.* Many attempts have been made to improve vision in this affection. Arlt says in respect to iridectomy which was recommended by von Graefe ('Arch. für Ophth.,' iv, 2, 271), that it can be of use only when the projection is slight and does not extend far towards the periphery of the cornea, or when the object is to use a latent portion for the attainment of tolerably good vision. He thinks it probable, from four cases operated on during the

first year of existence of this disease, that its progress may be arrested by early iridectomy (from the pupillary to the ciliary edge). A year later the disease was stationary in two eyes, and in the other two there was an improvement in comparison with the state before the operation (l. c., p. 344). Other methods, such as iriddesis (Critchett, Bowman), production of a contracting cicatrix (v. Graefe), excision of a small portion of the apex (Bader, Galezowski, Bowman, Wecker), have also been tried. It must, however, be admitted that all these proceedings are yet upon their trial.

Corelysis.—Mr. Streatfeild, who introduced this operation, says:

"If iris adhesions be few in number, three or four perhaps, and these be not wide, if the iritis have occurred but once, at least two or three months previously, and the iris seem otherwise healthy, able and ready to dilate, as much as could be expected, considering the adhesions, under the influence of atropine, and the latter remedy have been fairly tried (a drop of a strong solution placed in the eye three or four times a day for a week) and have failed, I have been in the habit of detaching them with a spatula-hook passed between the iris and the lens. There is no fear of wounding the lens. But, now, unless circumstances be altogether favorable, according to the above data, I am afraid of lighting up a fresh iritis, and of getting readhesions after the detachment has been done."[1]

The propriety of any operative proceeding is questionable in such cases, and the surgeon's decision will evidently be largely influenced by the greater or less importance attributed by him to a few narrow adhesions. von Graefe's views have been much exaggerated by some surgeons; what he asserted about numerous broad synechiæ has been extended to all cases. He considers the recurrence of iritis to be chiefly due to the former; yet the emphatic manner in which he speaks of the kind of adhesion seems to have been often overlooked.

"The existence of posterior synechia is in most cases the true cause of the tendency to recurrence. Iritis cured without posterior synechia seldom evinces any tendency to recur. Iritis with slight and extensible adhesions sometimes recurs, but in comparatively few cases. Iritis with many broad adhesions, resisting all artificial mydriasis, will very generally recur, and there is scarcely any exception to this rule when synechia posterior totalis—a perfect exclusion of the pupil by exudation—has occurred."[2]

On the other hand, excellent surgeons, as Arlt and Schweigger,

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[1] Loc. cit., p. 29.

[2] "Three Memoirs on Iridectomy," p. 252, New Syd. Soc., 1859. It deserves mention that v. Gräfe afterwards adopted the more correct appellation of annular synechia for these cases: see the 'Ophthalmic Review,' i, 23—25.

attribute scarcely any influence in this respect to adhesions that are insufficient to cause exclusion of the pupil. Arlt says (l. c., p. 346):

"It cannot be unconditionally admitted that the existence of posterior synechiæ is the chief cause of iritic recurrences. The synechiæ can be counted only among the causes of recurrence. Posterior synechiæ, even though broad and inextensible, cannot in and by themselves indicate iridectomy or any other operation; the inflammation must have already recurred, or at all events threatened (persistent or frequently recurring sensibility to light and exertion, pain or tension in the eye or its proximity, slight redness induced by occupation, change of temperature, or mental excitement): we should, besides, investigate the state of the other eye and general condition, and whether any consequent indication should be attended to previously or at the same time. *Iridectomy can remove only what may be termed a mechanically acting cause.* It will very often disappoint both us and the patient if we ask more.

"When, for example, a patient with chronic iritis and synechiæ is syphilitic, we should certainly not be content to perform iridectomy alone. Many sufferers from chronic, frequently recurring iritis show indisputable signs of scrophulosis; in other cases of the kind we have to do with people whose nutrition has been impaired by privation or weakening influences. (For example, irido-cyclitis after famine-typhus). Both eyes are attacked in such cases, one after the other, generally in striking symmetry, yet we are not usually justified in considering the affection of the second eye to be due to sympathy. I have been able to watch for years many patients on whom the operation has been performed for this reason. There has mostly been no subsequent attack. In many, however, there has been a recurrence once or more at some later period, notwithstanding a perfect operation and healing of the wound. The recurrent inflammation has been attended by pericorneal injection, discoloration of the iris, dotted opacities on the membrane of Descemet, and more or less haziness of the vitreous, whilst the coloboma, large and reaching to the periphery, has not changed. On the other hand, I have seen many patients for years (one from his 34th to his 55th year), whom I had treated for bilateral iritis, and in whom both eyes, not only the one iridectomised, but also the one with numerous synechiæ, have continued free from inflammation.

"Idiopathic iritis is closely related to another form of disease, the only difference during the attack and throughout their course being the frequent recurrence. Such patients are for an uncertain period liable to recurrences of iritis, the duration of the intervals and the severity of the inflammation being uncertain. We cannot then be surprised that as the rule they present a number of iritic adhesions. Yet this fact alone supports the assertion which is pretty generally admitted, that the synechiæ are the cause of recurrence.' 'The possibility of such a causal relation cannot of course be denied, yet the individuals with extensive posterior synechia who do not suffer

from recurrent iritis are too numerous for the mechanical influence of the adhesions to be blamed without more ado as the cause of recurrence, and on the other hand recurrence takes place quite independently of synechiæ. I have met with cases treated adequately by atropine from the very commencement, where no adhesions were left, and yet in which there were frequent recurrences without any assignable cause such as syphilis.'[1] I have repeatedly seen the very same."

Arlt is, consequently, not favorable to corelysis.

"I have hitherto preferred iridectomy when any operation was required. Even the excision of a small piece of the sphincter relaxes the parts fixed by synechiæ. Iridectomy, as we shall see in respect to glaucoma (§ 101), modifies at the same time the circulation of the uveal tract, and this, not to speak of medical and dietetic treatment, is certainly of more importance when 'there is a tendency to recurrence,' than the removal of any dragging, which may be imagined, but is, at all events, not directly proved" (l. c., p. 348).

Two slightly different methods of separating the adhesions, one by a very small incision, the synechiæ being torn by a hook (spatula-hook, Streatfeild), the other by a larger incision for the introduction of forceps (Passavant), have been employed. In addition to the older observations of Streatfeild, Weber, and Hasner, we may notice that Passavant operated more than fifty times according to the second method without any accident; he separated a single adhesion at a time ('Arch. für Ophth.,' xv, 1, p. 259, Berl., 1869). He has been followed by Mauthner ('Wiener Med. Presse,' quoted in 'Jahresbericht über d. Leist. und Fortschr. im Gebiete d. Ophthalm.,' 1er Jahrg., 1870, p. 303), who has operated many times and always with good results; and by A. Reuss (ibid., p. 304), who thus treated a case, in which there was a dense pupillary membrane, with the effect of improving the vision; a part of the edge of the pupil remained free and the membrane shrank considerably. Wecker, on the other hand, rejects the operation because he finds that it usually results in adhesion of the iris to the cornea ('Wiener Med. Wochenschr.,' quoted in the 'Jahresbericht üb. d. Leist., u. s. w.,' 2er Jahrg., 1871). To this Passavant replies that if his operation is performed exactly according to his directions, it has no bad effect.[2] Finally, we have two short but interesting communications to the American Ophthalmological Society ('Trans.,' 7th annual meeting, p. 67, New York, 1871; and 8th annual meeting, p. 130, New York,

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[1] Schweigger, 'Handbuch,' 1871, p. 321.

[2] Prof. Nagel (ibid., p. 304) has operated repeatedly with a good result. Twice the posterior was replaced by an anterior adhesion; in one case the latter was again easily separated. The iris sometimes readhered to the capsule.

1871). From these we learn that the operation was performed on nine eyes in eight patients; that there were twenty-four operations, and that more than one adhesion was separated on some occasions: that the adhesions numbered from one to at least five (in one case perhaps annular, and "pupil covered with a film of deposit"); they are thrice noted as broad or strong. The result was a free, movable pupil in six eyes, and in one it was circular; in respect to two the result is not stated. The only accident was the escape of some blood into the anterior chamber in one case.[1]

Iridodesis.—Adams (1812) and Himly (1816) recommended displacement of the pupil by means of a small prolapse left in the corneal wound (Iridenkleisis.) van Onsenoort (1822) punctured the sclera for this purpose.[2] It was recommended and often used by Tyrrell (see the chapter "of changing the natural position of the pupil," in his book, ii, 499, Lond. 1840) in conical cornea and central leucoma. Mr. Critchett revived the operation and rendered its immediate result certain by ligaturing the prolapsed iris. Thus modified, it became known as iridesis, iriddesis or iridodesis. It was employed in leucoma of the cornea, stationary partial cataracts, lateral displacement of the lens, and conical cornea. The operation in itself proved all that could be desired: thus, for example, the pupil was displaced from behind an opacity to opposite a clear portion of the cornea, and in many cases this was in each respect a decided gain. Unfortunately it has been found that serious diseases may arise from the synechia, even long after the operation. Such cases have been recorded by A. Graefe ('Arch. f. Ophth.,' ix, 3, p. 199), Steffan (Ibid., x, i, p. 122), M. Gruber (a case under A. Rothmund in[3] 'Deutsche Klinik,' 1866, p. 149, and 'Ophth. Review,' iii, 284), &c.[4]

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[1] There are some inaccurate statements in these papers. In the first Dr. Jeffries speaks of 13, and in the second of seven operations, whilst according to the details given there were 12 in each.

[2] He perforated the sclera with a needle, one line from the edge of the cornea, passed the hook along the *posterior* part of the iris till its point was seen through the pupil, and withdrew the pupillary edge of the iris, strangulating it in the wound. He says that he has repeatedly performed this operation with the best result; he has thus operated even on the two eyes of a patient. A. G. van Onsenoort, 'Genees-en heelk Handboek over de Oogziekten,' 2e deel., p. 454, Amsterd., 1840. Coloured plate of his first case in the 'Annales de la Médecine Physiologique,' t. ii, p. 97. Paris, 1822.

[3] Compare the account of the same case in Worlitschek's 'Mittheilungen.' 1866.

[4] Höring, "Three cases of Irido-Cyclitis after Iridodesis," in one case both eyes were lost, 'Klin. Monatsbl. für Augenh.,' 1865, p. 42, and 'Ophth. Review,' ii, 208. Rydel, "Purulent Iridocyclitis fifteen months after the operation," Tetzer, Rydel, and Becker, 'Bericht über die Augenklinik der Wiener Universität,' 1863-65, p. 80. Wien, 1867.

Arlt says (l. c., p. 345) that not only may purulent irido-cyclitis spring from the cicatrix months, even years after the operation, but what is still worse, sympathetic irido-cyclitis of the other eye may be excited. Streatfeild remarks:

"But by this method of proceeding the *ultimate* results are often very unsatisfactory: the *iris is confined*, and a low chronic recurrent iritis, &c., may be set up, which leads to far worse results than the state of things which the operation was intended to remedy. These artificial anterior synechiæ seem to be as pernicious as are the common iritic posterior synechiæ. We have recently seen many instances illustrative of this disagreeable truth. An excision of iris is a far safer though less attractive operation" (l. c., p. 24).

The consequence has been that many surgeons have entirely abandoned its use, and treat conical cornea, in which it sometimes gave very good results, by other means (see ante), and replace it in other cases by iridectomy or iridotomy.

Iridectomy.—The incredulity with which many surgeons received the announcement that this operation would arrest such diseases as irido-choroiditis and glaucoma seems to have almost disappeared. An examination of the principal treatises published and of the statistical tables of operations performed by various surgeons during the last ten years shows that the indications given are, with some slight modifications and limitations, generally followed.

What are these indications? What were the changes introduced by Von Graefe? "One of the first changes in the second period was the employment of the operation in the very large class of cases in which the patient can see perfectly with one eye, the other being more or less diseased. So long as the ultimate results of the operation remained very uncertain, no one would, of course, recommend its adoption, provided the patient's other eye was sound. When, however, better methods of operating had become more generally known, and success could reasonably be expected, many surgeons began to operate, also, in cases where the vision was perfect on the other side. Thus, Prof. A. von Graefe, who had adopted iridectomy in all cases as the only means of making artificial pupils with certainty, utterly rejecting division of the iris, or separation of it from its ciliary attachments, had, before 1856, performed it in more than 300 cases of blindness limited to one eye, the other being sound. The question may be considered as settled since the time when he published his paper ('Archiv Ophth.,' ii, Abth. 2, p. 193; and 'Med. Times and Gaz.' 1857, ii, 267), in which he showed, by an analysis of these cases, that in some persons there resulted binocular vision with its many important

advantages, such as the accurate estimation of distance; that in the less successful cases, the appearance of the patient was improved, and the field of vision enlarged; that even if squint or double vision occurred, they were quite amenable to treatment; in short, that it was always advisable to operate on the blind eye, the other being healthy, provided the artificial pupil could be made in a tolerably central position.

"In the very next paper in the same journal we meet with the celebrated article in which von Greefe recommended iridectomy in a great number of diseases, with an entirely different object from the one of allowing light to pass to the retina; and this essay was soon followed by two others, published in 1857 and 1858 respectively, in which he still further extended its applications. As translations of these papers have been published by the New Syd. Soc., it would be quite superfluous for me to do more than rapidly to enumerate the cases for which Graefe recommended this operation. Instead of taking them in the order in which they are mentioned in these papers, it will be better, I think, to classify them under four heads. 1. As a prophylactic, to prevent future attacks of iritis, or the extension of iritic disease to the choroid; to avoid the evil effects of the pressure of a swollen lens, either after cataract operations or in accidents; in certain cases, to prevent sympathetic disease of the other eye. 2. To relieve inflammation, or even only violent pain, as in some cases of choroidal inflammation, in iritis resisting other treatment, in corneitis where there is extensive ulceration, in sympathetic disease of a previously sound eye. 3. To diminish in some cases the intraocular pressure, as in glaucoma, staphyloma, &c. 4. As the best means of removing small foreign bodies impacted in the iris."[1]

The assertion that these indications are generally followed may be proved by a reference to almost any recent treatise, and scarcely calls for further remark. A short account of some statistics more particularly full and reliable, may illustrate the present practice. (See also Cohn, 'Vorarbeiten für eine Geographie der Augenkrankheiten.' Jena, 1874.)

Dr. A. Rothmund, junior, performed iridectomy 89 times out of a total of 420 operations in 1864: he distinguishes the results as good, moderate, or none.

		Good.	Moderate.	None.
For restoring vision	49	28	15	6
As an antiphlogistic	30	16	10	4
Preparatory to cataract-extraction	10	10	—	—
	89	54	25	10

[1] T. Windsor, "The Operation for Artificial Pupil: its history and present state," 'Brit. Med. Journal,' 1862, ii, 464.

It was performed as an antiphlogistic 7 times in inflammation of the iris and choroid, thrice for continually increasing posterior synechiæ, once in ulcer of the cornea, and 19 times in glaucoma (3 acute, 6 subacute, and 10 chronic). The result was always very good in acute glaucoma. In the other cases the vision was decidedly improved only 9 times. In one case of subacute glaucoma intra-ocular hæmorrhage took place during the night after the operation and ended in atrophy of the eye.[1]

Dr. A. Mooren performed 240 iridectomies in the year, 1865-66, out of a total of 665 greater operations, of which he gives the following table; the figures I, II, show that the operation was performed on one or both eyes:

	I.	II.
Leucoma corneæ centrale vel cicatrix adhærens	46	8
Præcipitata plumbea	1	—
Pterygium	1	—
Symblepharon	2	—
Ulcus rodens corneæ	1	—
Abscessus vel ulcus corneæ cum hypopio	13	—
Vulnus corneæ	2	—
Cicatrix corneæ ectatica	4	—
Perforatio corneæ cum prolapsu iridis	6	—
Corpus alienum in iride	1	—
Kerato-iritis	6	—
Iritis parenchymatosa	13	1
Irido-cyclitis	3	—
Irido-choroiditis	23	1
Irido-choroiditis sympathica	2	—
Choroiditis	3	—
Glaucoma	15	4
Cataracta accreta	6	—
,, traumatica	18	—
Iridectomia cum extractione cataractæ	2	—
,, cum extr. lineari per corneam	11	1
,, præparatoria pro extr. catar.	9	6
,, cum extract. catar. secundariæ	6	2
Total	194	23

The 240 iridectomies were performed on 213 eyes; in 9 cases the operation was repeated twice, in 3 thrice. In the first four diseases it was indicated for simply optical purposes.[1]

De Wecker in Paris had, in the second half of 1871, 61 iridectomies—28 optical, 33 antiphlogistic—out of a total of 248 operations:

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[1] K. Worlitschek, 'Mittheilungen aus der Heilanstalt für Augenkranke des Prof. Dr. A. Rothmund, junr zu München,' 1860.

[2] A. Mooren, 'Ophthalmiatrische Beobachtungen,' p. 179-181. Berlin, 1867.

		Results. Good.	None.
Optical—			
for opacity of the cornea with or without adhesion of the iris	23	19	5
„ simple closed pupil	4		
„ congenital cataract	1	—	—
Antiphlogistic—			
for partial staphyloma of the cornea	4	4	—
„ recurrent iritis	4	—	—
„ irido-choroiditis	15	—	—
„ acute glaucoma	1	—	—
„ simple chronic glaucoma	9	—	—

In 3 of the cases of choroiditis, patients who could hardly see to walk before the operation, could after it count fingers at twenty feet.[1]

The same surgeon had, in 1872, 157 iridectomies in a total of 630 more important operations.

Optical—	
Opacity of the cornea	31
Leucoma adhærens	11
Simple closed pupil	10
Congenital cataract	2
Post. polar cataract	2
	56
Antiphlogistic—	
Corneal abscess	1
Partial staphyloma corneæ	9
Recurrent iritis	8
Irido-choroiditis	30
Acute glaucoma	3
Glaucomatous keratitis	1
Chronic glaucoma	36
Wound of the cornea with adh. iris	10
Displacement of the lens	3
	101

Dr. J. Hirschberg, in Berlin ('Berl. kl. Wochenschr.,' 1870), performed 29 iridectomies in 25 patients; 4 times for glaucoma, thrice for abscess of the cornea, once for progressive sclerosis corneæ, 7 times for iritis, and the rest for leucoma corneæ and closed pupil. The same surgeon reports from, September, 1870 to September, 1872, 117 iridectomies; 72 antiphlogistic, 44 optical, and 1 for removal of foreign body from the iris. ('Klin. Beobacht. aus der Augenheilanstalt,' von Dr. J. Hirschberg, p. 110. Wien., 1874.)

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[1] "Relevé Statistique," par G. Martin, in the 'Annales d'Oculistique,' vol. 67, p. 157. Brux., 1872.

[2] "Clinique Ophthalmologique, du Dr. de Wecker, à Paris. Relevé Statistique," par le Dr. Georges Martin. Paris, 1873.

As to the performance of this operation in glaucoma we find that in the St. Petersburg Eye Hospital during the four years 1860-63 there were 95 cases of glaucoma with 158 affected eyes (39 men and 56 women) out of 1943 in-patients.

"The cases were generally treated by iridectomy—an operation which, indeed, was performed 101 times; once the eyeball was enucleated, and once paracentesis of the anterior chamber was employed. In respect to the curative action of iridectomy on the glaucomatous process, the results were perfectly satisfactory . . . The operation was performed fourteen times in glaucoma consummatum, when there was total blindness, simply for the purpose of relieving severe ciliary pains; 87 operations therefore remain, from which some action on vision could be expected. The results may be arranged in the following manner: Vision was improved 54 times; it remained just as before the operation 26 times; and 7 times it deteriorated after the operation, thrice from accidental cataract, the lens being wounded during the operation, and four times from advancing atrophy of the optic nerve."

The cases were thus distributed: Incipient glaucoma (premonitory stage) 10, acute glaucoma 12, chronic inflammatory glaucoma 57, glaucoma simplex 11, secondary or complicated 5, and eyes lost from glaucoma 63.[1]

Dr. L. Rydel has given a full account of the cases observed at the Ophth. Clinic of the Vienna University ('Bericht üb. die Augenklinik der Wiener Universität,' 1863-65, p. 132. Wien, 1867). There were in the three years 79 cases (46 men and 33 women); in 15 one eye was affected, in 64 both were diseased (or in all 143 eyes); of these 42 were totally lost. Iridectomy was performed in 79 cases, in 53 on one eye, in 22 on both eyes (or in all 97 times); 90 times with the view of preserving or improving vision, 7 times on account of violent pain, &c. The results were as satisfactory as could be expected with one exception. Many of the more interesting cases are related by Dr. Rydel at length.[2]

The use of iridectomy in partial staphyloma, glaucoma simplex, and exclusion of the pupil deserves some further notice.

Partial staphyloma of the cornea and projecting corneal cicatrices.—Beer observed so long ago as 1806 that the prominence sometimes receded after this operation. The same effect was noticed after him by Rosas, Heiberg, and Flarer. Chelius, jun. ('Staphyl. der Hornhaut,' p. 48, Heid., 1847) says positively that, if possible, an artificial pupil should be made in every

[1] See Dr. Magawly, "Contribution to the Clinical History of Glaucoma," 'Ophth. Review,' i, 233, from the 'St. Petersb. Med. Zeitschrift,' vi, 193, 1864.

[2] A series of cases of iritis, irido-choroiditis and glaucoma will be found in the appendix to Donders' 2nd report. Utrecht, 1861, pp. 277-420.

partial staphyloma. "By this means the partial staphyloma is changed into a simple synechia and any further increase prevented." Arlt says (l. c., p. 365) that a conical projection is more liable to be followed by secondary glaucoma than a spherical one, whether partial or total. Perception of light may continue for years in the latter; it is lost in a few weeks or months in the former. Von Graefe makes some important remarks in his last paper on glaucoma ('Arch. für Ophth.,' xv, 3, p. 108, and 'Ophth. Hosp. Reports,' vii, p. 92. Lond., 1871). He has seen by no means unfrequently cicatrices after central ulcers from blennorrhoic, diphtheritic, or pustular ophthalmia, where there had been no perforation, followed by expansion of the anterior chamber and ultimately by glaucomatous tension.

"Inclusion of the iris much intensifies the danger. For this reason it is of great importance to prevent synechia anterior in perforating ulcers and wounds, just as it is to avoid synechia posterior in iritis. As to both forms glaucomatous tension follows in only the minority, fortunately for the patients, yet both are attended by permanent risk in every case. We have noticed not unfrequently, that eyes affected with synechia anterior or leucoma adhærens, after being good for many, many years, are ultimately attacked by glaucoma on the approach of old age." "The greatest attention must be paid to all the objective symptoms, for these processes usually occur during childhood, when no great reliance can be placed on functional examination, and the effect of the treatment depends on the stage of the disease. It is indeed mournful how many eyes, after escaping entire destruction from ulceration, as in ophthalmia neonatorum, are afterwards rendered blind by secondary glaucoma, for which active treatment has not been used in time . . . No doubt it seems desirable for many reasons to postpone iridectomy in a child. Yet there is one urgent indication, never to be overlooked, owing to which the operation must be performed at any age; that is, an increase of the intra-ocular tension. Its first symptom in children with leucoma is usually an expansion of the anterior chamber. . . . To delay in leucoma adhærens after ophthalmia neonatorum till the increased tension can be estimated by the touch, would usually be to wait till too late."

Iridectomy in Glaucoma Simplex.

"Whilst the operation has a perfectly uniform effect in simple inflammatory glaucoma, whilst in secondary glaucoma its various results may be naturally deduced from the nature of the primary cause, we are obliged to admit that cases of glaucoma simplex vary extremely in their ultimate course, even when the pre-existing conditions appear to be exactly similar. The result is often very satisfactory in a relative sense, when the operation is performed at a late period; I mean that after six or seven years the patients continue to see decidedly better than at the time of the operation; in some, operated on at an early period, the vision remains for many years in the same state then gradually

deteriorates; in some, again, the disease is not rendered perfectly stationary; and finally, some, fortunately very rare, patients become rapidly blind after the operation. When we accurately determine and record the tension, we find that these differences more immediately depend upon its variations. Thus iridectomy reduces the tension directly and permanently to its normal amount in most cases of simple glaucoma. It reduces it considerably, but does not render it quite normal in others; the vision usually continues in the same state, the eye acquiring, as it were, a sort of adaptation to a moderate increase of tension; yet not unfrequently the sight becomes again worse after a time, or the pressure again increases slightly and gradually, until in either event by a second operation, which I shall afterwards describe, it is rendered permanently normal and the vision secured. In a third series, the tension is normal or but little increased soon after the operation, whilst at a later period there are from time to time considerable increases, which do not always permanently yield to a second operation. Lastly, iridectomy is sometimes followed by greater instead of less tension, the result being that vision is lost by progressive impairment or by a sudden attack resembling acute glaucoma.

"Hence it would almost seem as if iridectomy had such an uncertain effect in simple glaucoma as to render its indication doubtful. This conclusion is found to be quite erroneous when we consider the numerical distribution of the varieties described, It has been already mentioned that in more than half the cases, a single operation renders the pressure permanently normal, and not only preserves, but even gradually improves, the vision for a long period. The second category includes fully one fourth of the cases, and though our object cannot be said to be fully attained, yet the result is generally satisfactory; it may be necessary to repeat the operation in some few cases in order to secure permanent success. A second operation sometimes stops the progress of the blindness in the third category where the disease recurs; it at all events renders the deterioration slower in comparison with the spontaneous course. The last variety alone is thoroughly unfavorable, blindness being hastened by the operation. Yet this 'malignant' course is extremely rare; I have seen it fully developed only five times in some hundred cases of simple glaucoma on which I have performed iridectomy. Judging not only by my own practice but also by that of others, it does not form two per cent. of the cases of glaucoma simplex submitted to operation. Now an operation that gives permanent protection from blindness in more than 90 per cent., renders the course slower in the majority of the remainder, and does harm in less than two per cent., is indisputably indicated, though exceptional bad results, such as never occur in an analogous manner in inflammatory glaucoma, may necessitate prudence in the prognosis" (l. c., p. 99).

Arlt states that his experience is essentially the same (l. c., p. 358).

The iridectomy should be repeated when errors in the operation or after-treatment appear to cause the continuance or

recurrence of the glaucomatous pressure. The formation of a second and large coloboma at the diametrically opposite side is also advisable when the tension remains great or again increases after the first operation.

"Many comparative trials in the years 1865–67 proved that excision of the opposite piece of iris had a far more decided effect on the tension than that of the adjacent (even extremely large) portion of iris. These trials were made a few times when both eyes were in almost exactly the same state; the result was decisive, for excision of the adjacent piece had afterwards to be supplemented by that of the opposite portion of iris. I must indeed confess that I have become dubious whether excision of the adjacent piece has any influence over the tension; it is, at all events, very slight, and will generally not suffice in cases where the first iridectomy has had no sufficient or permanent effect. On the contrary, I could adduce at least a dozen cases where excision of the opposite piece has had a permanent effect after failure of the first iridectomy." (von Graefe, l. c., p. 106.)

"A man, æt. 53; left eye atrophied from glaucoma; right eye with good central vision, but great contraction of the field; in the spring of 1858 a violent inflammatory attack before the operation, which had been already arranged, was performed. The amount excised was insufficient, owing to the patient, who was not thoroughly under chloroform, moving when the iris was seized. The symptoms continued, though to a less degree. Operation repeated on the tenth day; patient more completely chloroformed. Coloboma enlarged downwards and inwards. Rapid diminution of the symptoms of pressure and inflammation, but very slow improvement in vision, which had sunk to mere perception of light. Even in the sixth week the patient did not dare to walk out alone. From the eighth week decided improvement, so that in the autumn Jäger No. 3 could be read with convex 10. From that time till now (autumn, 1873) no change whatever." (Arlt, l. c., p. 360.)

Posterior synechia.—Though the influence of isolated adhesions in respect to recurrences of iritis is not estimated so highly by many surgeons as it was by von Graefe, there is no doubt of the correctness of his assertion that—

"exclusion of the pupil is the point from which the further complications proceed, especially chronic choroiditis (with progressive amblyopia, and in the end atrophia bulbi). By exclusion of the pupil I understand simply the total adhesion of the pupillary margin with the capsule."[1]

Iridectomy is indicated in exclusion, *i.e.* posterior annular synechia, even though the centre of the pupil is clear, and the vision relatively good. It may also be employed, it may be indispensable, in blind eyes to arrest chronic inflammation of the iris and ciliary body, and to prevent further changes, even

[1] 'New Syd. Soc. Monographs,' p. 257. Lond., 1859.

sympathetic disease of the other eye. The prognosis is at first good; it becomes worse when (1) glaucomatous tension and excavation of the disc occur owing to serous exudation into the posterior chamber and vitreous body; or (2) when, on the contrary, the vitreous body shrinks from the very beginning or after some temporary increase of tension, and the retina separates from the choroid (contracting exudations on the inner side of the ciliary body and in the vitreous); or finally (3), when a layer of exudation between the capsule and iris unites them and the ciliary processes firmly together (total posterior synechia). In the latter the posterior chamber is entirely or almost entirely absent; it may be abnormally large in annular synechia. Occasionally the operation has to be repeated three, four, or five times before the disease is arrested, and a sufficiently large pupil persists.[1]

In respect to the complication first mentioned, Von Graefe says ('Ophth. Hosp. Rep.,' vii, 77):—

"Posterior synechiæ, the effects of iritis, are very often the cause of secondary glaucoma. As a general rule, we may say that the danger of ultimate increase of tension is proportionate to the extent and number of the adhesions, though occasionally glaucoma is excited by perfectly circumscribed, pointed synechiæ. The degree of danger is also influenced by various conditions, such as the age, with which it increases. On the other hand, the tension may remain normal throughout life; this is, indeed, numerically speaking, the rule even when the adhesions are numerous and extensive. As soon, however, as they *completely encircle* the pupil, causing exclusion, the tension seems to me to increase almost invariably. There must, of course, be no mistake; the most careful examination will sometimes fail to show minute apertures, through which fluid will pass. In my opinion, *projection of the iris by retro-iritic fluid is the only sign of complete exclusion.* When this state is clearly shown by the well-known prominences of the iris, glaucoma is sure to ultimately occur, and the patient is fortunate if it is not already present."

The use of iridectomy has, as we have seen, been somewhat restricted by the introduction of other operations, such as median incision of the cornea. It seems probable that it will be replaced in some cases by iridotomy. Wecker, who divides the iris with forceps-scissors, has frequently performed the operation and speaks strongly in its favour. The results of seventeen cases operated on by Prof. Rothmund have been recorded by Dr. Garvens ('Annales d. Ocul.,' vol. lxx, p. 123, 1873; vol. lxxi, p. 115, 1874; and vol. lxxiii, p. 118, 1875. E. H. Garvens,

[1] Arlt, l. c., p. 348. The case at p. 350, quoted evidently from memory, is somewhat incorrectly reported, if, as there seems no reason to doubt, it is the same as Case 17, in 'Bericht ü. d. ' Augenklinik der Wiener Univ.,' p. 72. Wien, 1867.

'Über die Iridotomie.' München, 1874; many historical details will be found in this pamphlet). Many operators avoid it also in the acuter stages of iritis and irido-choroiditis. Arlt expressly remarks, to prevent misunderstanding, that it should not be performed in iritis or irido-cyclitis whilst the inflammation is florid, but at least during a distinct remission. The only exception is when increase of tension forbids longer delay.

Its failure is well-nigh constant in sympathetic irido-cyclitis. Mr. Critchett[1] was the first to urge that enucleation is of little or no value when sympathetic ophthalmia has once commenced, that iridectomy does not arrest the disease, and consequently that operative treatment should be delayed till the inflammation has entirely ceased. Mooren, Stellwag and Schweigger are strongly opposed to any operation before the end of the inflammation. Mooren thinks that iridectomy in the progressive stage hastens the destruction of the eye.[2]

It would almost seem as if iridectomy was considered by some surgeons to be free from all danger, yet the fact is that, however well performed, it may be followed by destruction of the eye. Not to speak of such accidents as traumatic cataract, loss of vitreous, cystoid cicatrisation, which may be due to defects in the operation or after-treatment, we find it followed by purulent infiltration of the cornea (case of leucoma adhærens, 'St. Petersb. Med. Zeitschr.,' viii, p. 50, 1865; preliminary to extraction of cataract, Mooren, 'Ophth. Beobacht.,' p. 181, 1867; v. Graefe, 'Arch. für Ophth.,' xii, i, p. 214) and by panophthalmitis (arising from a cystoid cicatrix, von Graefe, 'Ophth. Hosp. Rep.,' vii, p. 92; in leucoma adh., Wecker, 'Clin. Ophth.' p. 6. Paris, 1873). Sympathetic disease has also occurred (Arlt, von Graefe). Mooren counts on an average $\frac{3}{4}$ to 1 per cent. of such unfortunate results (l. c., p. 181). Desmarres out of 161 ordinary cases of artificial pupil lost two eyes from chronic irido-choroiditis ending in atrophy, and had one case of phlegmon of the eye, whilst at the same time he had out of twenty-eight cases two of phlegmon from iridectomy in blind eyes ('Ann. d. Ocul.,' vol. xlvii, p. 221, 1862).

How can the effect of iridectomy be explained? Arlt thinks

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[1] 'Ophth. Review,' i, 178, Lond., 1865, from the 'Klin. Monatsbl. f. Augenh.,' i, 440.

[2] 'Sympathische Gesichtsstörungen.' See, on the opposite side, H. Müller in his thesis 'Zur Casuistik der Cyklitis,' p. 47-49, Greifsw., 1873. Over-estimation of iridectomy sometimes led to neglect of other treatment. E. D. Denis (Étude sur la Nature et le Traitement de certaines formes d'irido-choroïdite,' Paris, 1873) gives examples of the good effect of general treatment after the failure of iridectomy. He advises that, when due to a dyscrasia, it should be treated by appropriate means, any operation being of secondary importance.

scarcely otherwise than by its influence over the circulation in the iris and choroid. We have to account for two, apparently opposite, effects; in the one class of cases it causes a diminution in the intra-ocular pressure, whilst in the other, where the eye is abnormally soft, it is followed by greater, even normal, tension. He quotes some recent researches by Dr. Exner, Prof. Brücke's assistant ('Sitzungsber. d. k. Akad. d. Wiss.,' B. lxv, 1872, and 'Wiener Med. Jahrb.,' 1873). *Direct anastomoses between the arteries and veins* were found in the iris peripheral to the portion which had been excised two to four weeks previously (dog, rabbit). If, as in man, the iris is cut off on the pupillary side of the circulus art. irid. maj., the proximal portion of the larger vessels remains, whilst the greater part of their branches and capillaries is removed. The blood cannot circulate as before; large anastomoses are formed, probably by dilatation of previously existing minute vessels, so that the arterial blood passes at once into the veins, without the intervention of any proper capillary system. Should these researches be confirmed, the explanation would be easy of many practical points; thus a broad coloboma would increase the number of anastomoses, whilst total removal of the iris from its ciliary attachment would rather increase than diminish the intra-ocular tension.

THOMAS WINDSOR.

II.—On Inherited Syphilis.[1]

It may, we suppose, be stated as a general rule that our interest in a disease is, or ought to be, in proportion to the frequency of its occurrence; for the more common a disease, the greater is the amount of suffering it produces, the more often shall we have to attack it, and the more important therefore is it that we should understand its nature and its treatment. Inherited syphilis certainly has this claim upon our attention; but it has also another, quite as important, namely, its amen-

[1] 1. *Rare Cases of Congenital Syphilis.* By L. D. BULKLEY, M.D., A.M. New York. 1874.

2. *Cases of Disease of the Nervous System in Patients the Subjects of Inherited Syphilis.* By J. HUGHLINGS JACKSON, M.D. London. 1868.

3. *On Dactylitis Syphilitica, with Observations on Syphilitic Lesions of the Joints.* By R. W. TAYLOR, M.D. New York, 1871.

4. *On Pseudo-paralysis due to a Lesion of the Bones in Infants, the Subjects of Inherited Syphilis.* PARROT, in 'Archives de Physiologie,' 1872.

5. *London Medical Record,* vol. i, p. 10. *Summary of Contributions to the Knowledge of the Pseudo-paralysis of Inherited Syphilis.* By S. GEE, M.D.

ability to treatment. It is one of the commonest diseases affecting the children of the poor; but fortunately, if we except the most aggravated cases, it is also one of the most curable; wherefore, it seems very desirable that we should be able to recognise certainly all its manifestations, which are many and diverse; and it may be useful to review briefly our present knowledge of the disease, and especially to draw attention to some of its rarer and less recognised symptoms.

It will be well to premise that our remarks will be confined to *Inherited* or *Congenital* syphilis, that is to say, to that variety of the disease which is communicated to the ovum in utero, and will not concern the *Infantile* variety, which is acquired by the infant, either in its passage from the uterus into the world by contact with sores on the mother; or after birth, by other means of inoculation, such as contact with sores upon a nurse, or vaccino-syphilisation; which is in fact ordinary acquired syphilis occurring in a child, and is a comparatively rare disease. We have said that inherited syphilis is a common disease. This is certainly the fact in London, for a large proportion of the children attending as surgical out-patients at the hospitals are suffering from some form of the disease; but it is right to modify this assertion by stating that there is a remarkable absence of the affection among the children in many of the manufacturing districts of the north of England. Then again it is doubtless more common among the children of the poor than among those of the rich, because the poorer classes are less careful to get cured of syphilis, and probably more often marry while under its infection; also because their poverty, and its attendant deprivations and lack of hygiene, renders them more vulnerable to the disease, and allows it to run its course with less restraint. Nevertheless it is sufficiently common among the children of the upper classes, and is apt in them, probably for the reason that it is to a greater extent modified by the treatment of the parents, to present more of the uncommon forms of the disease. And without touching upon the general questions involved in the application of the Contagious Diseases Act, we cannot help expressing our opinion that a strong argument in its favour is afforded by the consideration that as long as syphilis is allowed to run rife, thousands of innocent children must every year be born, either destined, after a few weeks of wretched existence, to a premature, though in their case not to be lamented death, or with the miserable prospect of years blighted by the sufferings which pertain to one of the most dreadful of diseases, and of carrying to their graves the indelible marks of their direful inheritance.

As, in doubtful cases, the diagnosis of inherited syphilis is

greatly aided by the condition and history of the parents, it is very necessary to bear in mind the several sources and methods of infection. These we briefly state.

1st. It may be derived from the father, who, being the subject of syphilis, conveys the disease to the ovum by means of the semen. 2nd. From the mother, by means of the placental circulation, whereby the infected blood is conveyed from the mother into the system of the fœtus. 3rd. Both parents may be syphilitic, and therefore both the above-named modes of infection take effect. And it may here be remarked that the reverse of the second method of infection may take place, that is to say, a healthy mother may, by means of the placental circulation, be infected by a syphilitic fœtus, which derives the disease from the vitiated semen of the father—an occurrence which explains the cases in which a woman who has never had primary syphilis, may exhibit secondary symptoms after nourishing an infected embryo.

The fact that syphilis can be conveyed to the offspring by inheritance seems to have been first definitely stated by Paracelsus in 1529, though before that time obscurely hinted at. Very few facts, however, were observed or recorded, and these were soon lost in the clouds of speculation in which the period was so prolific, until Boerhaave in the beginning of the eighteenth century shed the light of his genius upon the subject, and once more revived, or rather perhaps originated, the intelligent study of the disease. Since then it has been the subject of much careful investigation both in this country and on the Continent, and though every year brings us some fresh knowledge concerning it, yet it still remains a disease affording wide scope for fruitful study.

The malignity of the disease is in proportion to the intensity of the infection, and this is affected by two causes: first, the mode of derivation; secondly, the period of the disease in the progenitor. As to the mode of derivation, that is, whether from father, mother, or both parents, the rule is that the infection is most intense if both parents are diseased; that disease of the mother alone is the next most potent cause; and of the father alone the least. And as to the period of the disease in the progenitor, it is well established that the more remote the time of impregnation from the period when the parent first contracted the disease, the less profound will be the infection of the children. This is the case even independently of treatment. Thus a common history is such as the following. A woman the subject of syphilis had three children born dead; at the fourth conception a living child was born, but so powerfully infected that it soon died; the fifth child was less diseased and by care

and treatment was reared. Should there be other children they may each be less infected until the latest may only present one or two symptoms, which in the absence of the history, would hardly be recognised as syphilitic. In the majority of cases the symptoms of inherited syphilis do not appear until a few weeks after birth, although the child often seems feeble and the skin of an unhealthy hue: there is no doubt, however, that a child may be born with symptoms of the disease. The greater number appear healthy at birth, and first show symptoms of infection about a month afterwards. As Trousseau remarks, it is unusual for the symptoms to occur before the second week, or, *for the first time*, after the eighth month. But it is important to remember that a child who has been in apparent health for months, or even years, may suddenly show symptoms of inherited syphilis, it may be thought for the first time. We have lately seen a girl of 14 years in whom this occurred. But on investigation it will almost always be discovered, as in the case alluded to, that the child had some manifestations of the disease soon after birth, to which perhaps but little importance was attached, or which soon subsided under treatment, and which having lain latent for a long time, were only again educed by some depressing influence, such as one of the acute specific diseases of childhood, bad hygienic condition, or the like.

It is not necessary to enter upon a description of the ordinary symptoms of inherited syphilis, which are sufficiently well known; but there are a few of these which we think are of greater value than the text-books would lead one to suppose. For instance, there is none more striking than the physiognomy of the infant. The physiognomy of disease generally is a subject well worth study; every one must recall instances in which he has diagnosed from the face alone, cases of peritonitis, phthisis, chorea, and other affections; and in no disease is the facies more characteristic than in inherited syphilis. Trousseau has given of this, as of so many other diseases, a most graphic description. He describes the complexion as of a bistre tint; and there is a striking look of premature old age about these infants; the skin too is shivelled, the body emaciated, and often there is a peculiar smell about the patient, for which we know of no better comparison than that of a damp vault.

One of the earliest and most constant symptoms is what is known among the poor as "the snuffles," that is, a thick discharge from the nasal mucous membrane, which blocks up the nares and causes a snuffling with the respiration, and a great, and sometimes serious obstacle to sucking. It also produces an alteration in the voice, so that the infant's cry has a peculiarly high-pitched or twangy sound. Then there are very constantly

sores about the nates, flat mucous tubercles, or round copper-coloured, rather shining spots having a tendency to desquamation. Mucous tubercles occur also about the organs of generation, the mouth, the genito-crural fold, the axilla, between the fingers and toes, between the chin and lower lip; in fact, as Diday has remarked (giving the true explanation of their character) in any part where the skin is "thin, moist, and exposed to friction." The same kind of eruption takes place on the mucous membranes, and if the mouth is examined, there are frequently found white patches or ulcerations of a round or crescentic form, scattered about its lining membrane. A favorite position for these is the point of reflexion of the mucous membrane of the lips on to the gums, and the frænum of the upper lip; another is the border of the isthmus of the fauces. The extension of this eruption to the larynx gives rise, according to Diday, to another symptom frequently met with, namely, hoarseness, not the high-pitched note associated with snuffles, but a true hoarseness, very unlike the usual note of an infant's cry. Diday says this depends upon the eruption specially affecting the edges of the aryteno-epiglottidean folds.

Of the other eruptions the most common is a roseola, and this has less of the coppery hue than the other syphilides of infants. It chiefly affects the chest, neck, and inner part of the thighs, is an early symptom, and is distinguished from the other exanthemata, such as scarlatina, but not being accompanied with febrile disturbance, by its imperfect disappearance on pressure, and "by its maintaining the same colour and degree until its termination." On the palms of the hands and soles of the feet there is commonly desquamation, giving an appearance of psoriasis. This is very characteristic of inherited syphilis. Besides this there is a kind of intertrigo seen very often, which differs from the ordinary intertrigo in its surface having a less bright red colour, but a much more shining or polished appearance, accompanied by a tendency here and there to slight desquamation. Eczema and impetigo are also not unfrequent symptoms, and are each of them of a darker tint and less acute character than when unassociated with syphilis. There has been much discussion as to whether or not the pemphigus of new-born infants is a syphilitic eruption. We are hardly able to assert that it is invariably of syphilitic origin, but certainly the subjects of it usually present other symptoms of inherited syphilis. Such a case as that related by Dr. Bulkley, in which the eruption continued to develop for nearly a year, and the child died of syphilis, is very strong evidence of its syphilitic character.

Another very common symptom is the existence of fissures or cracks at the edges of the lips and nares and the angles of the mouth,

the scars of which are often valuable evidence of former mischief. The hair often falls off; so do the eyelashes, leaving a sore edge to the lids.

Among the more important of the consequences of inherited syphilis are the visceral lesions. These occur very early in the disease, are mostly of the suppurative type, and progress very rapidly. It is these which account for the majority of the deaths from inherited syphilis. They differ entirely from the syphilitic visceral lesions of adults—the results of acquired syphilis—which occur in an advanced period of the disease, are very slow in progress, and are not suppurative. Very little has been added to our knowledge of these since the publication of Diday's well-known book, in which they are fully described. In the lungs they consist of scattered abscesses surrounded by compact yellowish-grey tissue—the symptoms are those of lobular pneumonia. Disseminated suppuration of the thymus gland is another of these syphilitic affections. Of the liver, the lesion consists in enlargement and induration of the organ in whole or in part, due to the development of fibro-plastic material between the cells of the acini, with obliteration of the vessels, and interference with the secretion of bile. The organ is very elastic; a section exhibits " on an uniform, yellowish ground a more or less close layer of small white opaque grains like sago." On pressure no blood is forced out, but only a yellowish serum; the general colour is sometimes of a brownish-red tint and the structure ill-defined. This condition is generally developed during uterine life, and is rapidly fatal. The symptoms are vomiting, diarrhœa, and tympanitis; but, strange to say, no jaundice. The enlarged and indurated organ may be felt by palpation. It is probably in connection with this condition, that the peritonitis described by Simpson as occurring in inherited syphilis is found. Diday does not mention any affection of the spleen, but a condition very similar to that found in the liver is not very uncommon. Dr. Bulkley has described a case which he supposes one of syphilitic disease of the lung in an infant, and considers that it was probably of the kind described by Lorain and Robin as "white hepatization," and quotes Thierfelder's description of that disease. It is as follows:

"The small air-cells are separated from each other by very broad bands of interstitial tissue, which consist, in addition to a small amount of connective tissue and a few spindle-shaped cells situated mostly very near the vessels, of irregularly arranged masses of cells and nuclei of syphilitic neoplasm, much the same as observed in syphilitic laryngitis. This tissue is distributed in the lungs of infants either in the way of general infiltration of the septa, leaving but small air-interstices; or wedge-shaped collections are formed, generally near the pleura,

embracing perhaps a lobule, which have to the naked eye the appearance of firm, bluish-red masses resembling atelectasis."

The infant alluded to by Dr. Bulkley died, however, when less than four months old, and we do not think that as a rule the "white hepatization" occurs at so early an age. There was unfortunately no post-mortem examination, but as the child had ulcerations of the throat and laryngeal symptoms, it seems more probable that the mischief extended to the bronchi and gave rise to lobular pneumonia. Dr. Hughlings Jackson has recorded some cases of nervous diseases in connection with inherited syphilis, and suggests that plugging of minute branches of the cerebral arteries may account for some of these; and that convulsions, hemiplegia, and even chorea may thus be caused. There is at present, however, a lack of pathological evidence on this point, which is nevertheless well worthy of further investigation.

If the child lives long enough there are other manifestations of the disease which it is important to recognise. One of the best known of these is the characteristic condition of the upper permanent central incisor teeth, first described by Mr. Hutchinson. These teeth are ill-developed and therefore separated, soft, and marked with a crescentic notch. Moreover the liability to caries is such that even the temporary teeth become worn to a peg-shape. Mr. Hutchinson has also shown the connection of interstitial keratitis with inherited syphilis. These cases are very common, and we think the symptom a very valuable aid to diagnosis. The cornea becomes of a diffused opacity (and the sight is proportionately interfered with), but does not exhibit the *patches* of opacity met with in scrofulous ophthalmia. Retinitis and iritis also occur, but much less commonly than in acquired syphilis.

The disease of the nails described by Mr. Hutchinson as dependent upon inherited syphilis is, we think, much less common and characteristic than the condition of the cornea and teeth.

Deafness, ulcerations of the pharynx and tongue, laryngitis, and lupus are also among the less common of the later manifestations of the disease. We remember to have seen an unfortunate child who was very nearly blind from interstitial keratitis, quite deaf, greatly disfigured about the face from lupus, and in whom tracheotomy had been performed for laryngitis—a sufficiently striking example of the effects of inherited syphilis. Warts on the mucous surface are of not uncommon occurrence. They are found chiefly upon the hard palate and back of the tongue, and at that stage of the disease when the keratitis occurs. They are usually multiple, large, and sessile.

Another condition dependent upon inherited syphilis, which, though not uncommon, we have not seen anywhere described, is a circumscribed chronic cellulitis. This is one of the later symptoms and is often found in connection with the notched teeth and interstitial keratitis. It is characterised by the occurrence of limited indurations of the subcutaneous cellular tissue. They differ from the commencement of a subcutaneous abscess in that they are defined, not painful, nor hot; the skin over them is natural, they often remain stationary for a long time, are unaffected by local treatment, but generally eventually soften and degenerate into a material which can hardly be called pus, but is composed of the *débris* of the connective tissue. When this has occurred the skin slowly becomes thinned and ulcerated, and one or more openings occur whereby this *débris* escapes, leaving a portion of the skin undermined, and an indolent ulcer upon the surface. These indurations are defined, often of linear shape, not generally extending to more than two or three inches, and when cut into appear to be due to a chronic cellulitis, and not to any specific growth or infiltration. Their favorite sites are the buttocks and outer and back parts of the thighs and legs, and, to a less extent, the arms. Attention has been called during the last few years to a disease of the bones dependent upon inherited syphilis, which, though by no means uncommon, is not usually described among the symptoms of the disease. It has been called by Parrot, whose description of the disease we have had opportunities of confirming, the "pseudo-paralysis of inherited syphilis." The name gives an idea of the most striking characteristic of the disease. The child is usually brought to the surgeon on account of inability to use some of its limbs, which hang down as in paralysis or fracture; or are held fixed as in articular disease. Sometimes almost all the limbs are affected and the child presents a most helpless appearance. Parrot says that there are often large abscesses in the neighbourhood of the joints, which are then fixed and bent, and that crepitus can sometimes be felt between the shaft and the epiphysis of the bone. These symptoms depend upon an inflammatory affection of the end of the shaft of the bone, and of the ossifying layer of the epiphysial cartilage, whereby the epiphysis is often separated from the diaphysis. The ends of the bones become thickened externally and softened within; and may eventually be destroyed by a puriform infiltration leading to destruction of the lamellæ and the formation of cavities. The joint is not usually involved, the muscles and nerves are unaffected. Several bones are always affected, and there are usually other well-marked symptoms of inherited syphilis. The order of frequency in which the bones are affected is, according to Parrot,

as follows: the femur, humerus, tibia, ulna, radius, fibula, ribs, ilium, scapula, clavicle, tarsal, and carpal bones. This disease is definite and not easily mistaken, and we have no doubt correctly attributed to inherited syphilis, of which it is one of the earliest symptoms. We think, however, that suppuration is of less frequent occurrence than would be supposed from Parrot's description. The disease yields rapidly to mercurial treatment.

Dr. R. W. Taylor, of New York, has laid much stress upon an affection of the fingers and toes, which he considers to depend upon inherited syphilis, and which he describes under the name of "dactylitis syphilitica." It consists of an enlargement of the phalanges, due to the growth of a lowly organized form of connective tissue in the soft parts around the bone, and an inflammation of the bone itself. The swelling is chiefly on the dorsal aspect of the bone and greatest about its middle, so that the phalanx has a bulbous appearance, and somewhat of a wedge-shape on section. Dr. Taylor alludes to cases reported by Nélaton, Archambault, Lüche of Berne, and others, and Dr. Bulkley has also reported two cases. We do not think the syphilitic character of this affection is by any means clearly established. In the first of Dr. Bulkley's cases, for instance, which is quite a typical one, there is no evidence of syphilis either in the child or its parents; and in his second case, though the mother was syphilitic, the child gave no sign of infection. Dr. Bulkley quotes a third case, which he himself considers to be of traumatic origin, in a scrofulous child, but in which we fail to discover any distinction from the preceding cases, which are said to be syphilitic.

Dr. Taylor says that these cases are to be distinguished from scrofulous dactylitis by their chronicity, and the absence of pain, but we should say that these are precisely the characters of scrofulous affections; and he moreover asserts that scrofula rarely if ever attack the phalanges—a statement from which we entirely dissent. Diseases of the osseous system are usually said to be rare in inherited syphilis, but we think are really more common than is supposed; but that as they are most often met with as a late symptom, when the other manifestations of the syphilitic infection may have passed or been cured, their character is less well recognised. We have ourselves seen cases in which children suffering from bone caries had apparently no sign of inherited syphilis; yet in whom we had had the opportunity of watching the usual series of syphilitic symptoms from birth upwards, and in whom the rapid recovery under anti-syphilitic treatment rendered the diagnosis complete. And we have certainly seen a large number of children suffering from nodes upon, and caries of the bones, the undoubted result of

inherited syphilis. It is the more important that the character of these cases should be recognised, as they are unsatisfactory subjects for operative treatment, but as a rule rapidly recover under anti-syphilitic medicines. Of the treatment of inherited syphilis but little need be said, for it resolves itself chiefly into giving mercury in the early stage, and in the later stages iodide of potassium, with or without mercury.

There has of late manifested itself in the profession a growing scepticism of the powers of medicines; a reaction, as we think, such as we see in so many other matters, from the unreasoning and sometimes unreasonable belief in their virtues which preceded it: but none the more founded upon intelligent observation and deduction. But we think no one who has watched the effects of mercury given in cases of inherited syphilis, ought to doubt the potency of at least that drug. We know few things in therapeutics more satisfactory than to witness the improvement of children thus treated; they rapidly fatten, lose their shrunken aspect, and change from little old men, to actual infants; the eruptions fade, the sores heal, and the progress is often astonishing. We do not of course assert that this is the case in all; there are many children so profoundly affected by the disease that they die before remedies have time to act; or they may be born with fatal lesions already developed. But we cannot doubt that we possess great power over this disease, a fact which alone must always give an interest to its study, and an importance to its recognition.

III.—Fox on Pathology of Nervous Centres.[1]

The knowledge we possess of the normal structures of the nervous centres, and our acquaintance with the pathological conditions of these organs, are derived from researches that have been prosecuted within the period of the present generation. So rapid have been the advances in neuro-physiology and pathology since Stilling and Schroeder van der Kolk first pointed the way, that the results of the labours of their followers have left the text-books of their days so far behind as to be now almost obsolete, if not mere vestiges of a byegone stage in the science of medicine. To a great extent, however, the vast mass of information that has accumulated on our hands is distributed over

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[1] *The Pathological Anatomy of the Nervous Centres.* By Edward Long Fox, M.D., F.R.C.P. London, 1874.

the pages of our periodical literature, and is daily being added to in the same, we might almost say, discursive manner. Thanks, therefore, are undoubtedly due to any one who may exhibit sufficient industry, and possess a competent knowledge of so wide a range of subjects, as to be enabled to bring together into a focus the light otherwise so dispersed. Such a task Dr. Fox has undertaken, and, we may add, has successfully achieved. He has presented us with the first complete treatise on the pathological anatomy of the nervous centres that has appeared in this country.[1] We would not be understood to imply that Dr. Fox's treatise is simply a compilation—on the contrary, it has the merit of being, to a large extent, the result of personal observation and original investigation, carried on during seventeen years of hospital work in Bristol.

This treatise is divided into two parts; the first describes the pathological anatomy of the brain and spinal cord; the second is concerned with the mode in which pathological results are grouped in certain conditions to which symptomatically have been given special names, as mania, melancholia, &c.

"This division," Dr. Fox remarks, "is simply for the sake of convenience. Its imperfections are manifold; specially because in many instances it is logically a cross-division. As an example, in the first part, the fifth section, on tumours, stands by itself without any connection with the second section, on abnormalities of the vascular system. Yet tumours of the brain and spinal cord are pathologically divided into those connected with the membranes, those having their origin in the walls of the vessels, and those springing from the neuroglia, the connective tissue of the brain and spinal cord."

Dr. Fox submits a classification of the lesions of the various anatomical constituents, which, although it involves a certain degree of tautology, is certainly, speaking pathologically, a more logical division of the subject.

As it would be impossible fully to do justice to the whole contents of this volume of lectures, we propose to set before our readers a few points whence we think they may judge of the entire work.

We take, as occurring early in the volume, the section on cerebro-spinal meningitis. This affection, sometimes sporadic, sometimes epidemic, presents different characters in different cases. In some instances there has been scarcely any traceable changes in the organs, whilst in other cases there has been extensive exudation both within and beneath the arachnoid.

[1] An elaborate German work, embracing also the normal anatomy of the nervous system, is now in the course of publication, viz., 'Allgemeine Pathologie der Krankheiten der Nervensystem,' von Dr. Gustav Huguenin, Professor in Zürich.

Dr. Fox states that in the cords he has examined he only once found evidence of disintegration, and adds that specimens of local softening have been shown by Drs. McDowel and Hayden, of Dublin.

"Softening of the substance of the encephalon, though also uncommon, appears to be more frequent than the corresponding condition of the cord. Allusion is here made only to cases of white softening, but Klebs, on two or three occasions, in rapid cases of cerebro-spinal meningitis, has found foci of softening, varying in shade from straw-colour to red in the centrum ovale, and in each of these cases he has found indications of recent endocarditis."

In connection with the description here given, the following sporadic case by Dr. S. T. Dowse[1] may be quoted. The patient was a female aged 26 years, having a syphilitic history. Through the whole course of the disease, about six weeks, the sensorium was unaffected. Acute muscular pains, formication, and paralysis occurred. The special senses were not impaired. The temperature was as high as 105°, whereas in the epidemic form it rarely exceeds 100°. There was an absence of reflex irritations and convulsive spasms, due in all probability to rapid change in the grey matter.

"The substance of the hemispheres, as well as the central ganglia of the lateral ventricles, was healthy; the latter contained a normal quantity of fluid. Upon exposing the base of the brain to view, it gave at once the characteristic smell and appearance of gangrene. Over the surface of the anterior lobes, as well as over the middle and posterior, the arachnoid membrane was thickened and of a semi-opaque appearance, but in the immediately central line over the parts forming the floor of the third ventricles, and on either side in connexion with the middle lobes, pons Varolii, crura, and medulla oblongata, it was thickened, opaque, and fibrillated. Over the surface of the lobes of the cerebellum, on each side of the medulla, the membranes, both pia-mater and arachnoid, were striking, and of a dirty green colour. Upon their removal, the brain-substance underneath was found to be softened to the depth of a quarter of an inch, and presented the same characteristic features, and so did also the parts forming the floor of the fourth ventricle. The same condition pertained to the parts forming the floor of the third ventricle, as well as the corpora quadrigemina and geniculate bodies. The right and left crus were considerably disorganised, and of a greyish-green colour to the depth of about half an inch. The arteries forming the circle of Willis, as well as the anterior inferior cerebellar arteries, were firmly bound down by inflammatiou. The substance of the pons and medulla were apparently healthy. The cord with its membranes when removed weighed fifteen drachms. Upon slitting up and reflecting the dura mater, the anterior surface of the cord was found coated in the cervical and dorsal regions with a layer

[1] 'Lancet,' Jan. 31, and Feb. 7th, 1871.

of thick, purulent, lymphoid, corpuscular material; it presented, like the brain, a stinking odour, and in parts a greenish colour. The arachnoid and pia-mater could not be separated; they were adherent to the substance of the cord itself. All hyper-vascularity, if it had existed, was only to be found at the extreme ends of the cervical portion on the one hand, and the lumbar on the other. Between these extremities the substance of the cord differed in different parts. In the dorsal region it was considerably softened, and about the origin of the sixth, seventh, and eighth pairs of nerves it was of the consistence of thick cream, and presented a mottling of a greenish-blue colour, and a putrid smell. In the cervical and lumbar regions, where the cord had undergone the least change, the posterior columns were found to be comparatively healthy."

Dr. Fox proceeds to treat of hydrocephalus, and of myelitis. Under the latter he discusses the inflammatory origin of certain forms of degeneration.

Under the head of degenerations he includes atrophy, softening, and sclerosis.

"The distinctions between these varieties," the author admits, "are arbitrary, and can only be defended on the ground of convenience. Thus atrophy may depend upon or be intermingled with softening, or again, with sclerosis. It may be due to inflammation, as also may at least one variety of softening, and it is an open question whether all forms of sclerosis do not own a similar inflammatory origin."

This question is affirmatively answered by some Continental pathologists, who place all forms of sclerosis under the category of *myelites chroniques*, parenchymatous, and interstitial.[1] Atrophy also is usually found to be associated with the evidences of inflammation; "thickening of the meninges, and patches of meningeal ossification, with fluid in the ventricles, or, if the atrophy is unilateral, with fluid in the ventricle of the affected hemisphere, with thickened ependyma, and indurations of the ventricular wall. . . The process in order of sequence being inflammation, then softening with fatty degeneration, then partial absorption and so atrophy."

Atrophy of the brain, however, may take place without the agency of inflammation. The cells and nerve-tubes will be found shrunken. The vessels will have undergone also a shrinking from the surrounding brain substance in which the nutritive processes having fallen short, an atrophied condition has been the consequence.

Atrophy of the cord, Dr. Fox points out, will be found to depend either on atheroma of the smallest vessels, or on myelitis, most usually chronic, or on grey degeneration, or general or partial sclerosis, disseminated or non-disseminated.

[1] "Sur les Myelites," par M. Clement, 'Archives de Médecine,' 1874.

The opposite condition, hypertrophy of the brain, is to be regarded as merely a hyperplasia of the connective tissue. There is, observes Dr. Fox, no real hypertrophy of nerve-cells. It may, however, be doubted whether this proposition can be maintained—we have seen some of the multipolar cells of the spinal cord so markedly large, when compared with others near them, that we could but consider that they had undergone a hypertrophic change.

Dr. Fox gives a very full and exact account of the various forms of softening of the brain, describing the diagnostic differences of red or acute, from those of the chronic or white softening, and of the intervening stage of yellow softening, with their relation to the phenomena of embolism. In like manner the author presents us with a complete summary of the views at present entertained upon the nature of the wide-spread lesions spoken of as grey degeneration, sclerosis, &c. These terms, he observes, are used variously by different writers. To some they express various conditions, to others they are one and the same thing; to others, again, we believe, they convey no intelligible idea. Dr. Fox quotes the classification of Rindfleisch, with reference to these pathological conditions, viz. 1, simple non-inflammatory—grey degeneration proper; 2, indurating inflammatory form—sclerosis.

Grey degeneration Dr. Fox states to consist of "amorphous matter, finely granular, between the nerve-tubes, disappearance or atrophy of the latter, numerous nuclei of connective tissue, amyloid bodies, and fatty granulations." What these last may be, or how they may be distinguished, we are at a loss to know. It is nevertheless a pathological condition of which we find frequent mention by Continental writers. The lesion represented thereby we suspect to be the miliary form, that has been described by some British pathologists. Several well-drawn illustrations accompany the author's description of this lesion. These give a very exact representation of the spots of colloid and amyloid degneration, as well of the miliary and diffused forms of sclerosis.

These forms of degeneration, we agree with the author, are very frequently found together in the same case, and indeed seem in some instances so to run one into the other that it would not be easy to distinguish grey degeneration, colloid, and the so-called miliary sclerosis, from each other. The description of grey degeneration will apply, almost *verbatim*, to that of miliary sclerosis—viz. excess, and proliferation of connective tissue; compression thereby, and consequent degeneration of the nervous filaments, accompanied with a granular degeneration of the neuroglia.

"The only anatomical distinction," Dr. Fox very rightly remarks, "that can be drawn between sclerosis and grey degeneration is in the greater or less proliferation of this fibrous network, and in the fact of the presence of corpora amylacea in grey degeneration. It is wholly a question of degree, and I am loth to agree to the one being inflammatory and the other not. It is probable that both are of inflammatory origin."

The observations of Rindfleisch are quoted as confirmatory of this inflammatory origin of sclerosis, viz. that sclerosis starts from foci having in the centre of each a distended blood-vessel cut across. That traces of this condition may sometimes be met with we do not dispute, but that it is the mode of commencement of the change in the miliary form, our own examinations have not confirmed. On the contrary, the spots of sclerose degeneration may be traced from their largest patches down to minute amorphous bodies presenting no contained structures. That their origin is in nerve-fibrils, or degenerated corpuscles of the neuroglia, is much more consistent with their microscopic characters.

The miliary and the grey degeneration are much less frequently met with in the grey matter than in the white substance—the latter appears to be its special seat, and only in instances of extensive degeneration is the grey matter either of the cord or brain invaded.

Sclerose en plaques is a form of lesion so called by French pathologists, more correctly designated "insular sclerosis" by Dr. Moxon. It is met with both in the brain and spinal cord. Clinically its presence may be inferred by paralysis with a peculiar tremor of the muscles, as in paralysis agitans. Microscopically, it is similar to the condition above described,—proliferation of nuclei of the connective, hyperplasia of the reticulated fibres of the neuroglia, with degeneration of the nerve-elements. Having thus described the lesions that constitute the various forms of sclerosis, Dr. Fox very aptly adds,—"It must be remembered that the aggregate of symptoms does not depend on the special form of invasion of the disease, but on the foci which are attacked; and when we come to speak of other diseases, you will find there is in many cases no broad line of demarcation between them and cerebro-spinal sclerosis."

As it is in chronic forms of mania that the degenerations here referred to have been very frequently met with, it may form a profitable sequence to the previous remarks to follow Dr. Fox in his observations on insanity, and to endeavour to see how far his pathological researches bear out the statement sometimes hazarded that in the majority of cases of insanity no change can be detected in the structure of the brain. To most of such statements we should be inclined to oppose the worthlessness

of any other than microscopical examinations, admitting at the same time the possibility that appreciable alterations may have existed, and have disappeared with life.

"There are," observes Dr. Fox, "at least four conditions that may induce very serious cerebral symptoms, and yet leave little or no change to be recognised after death: 1, a condition of the blood itself, as in uræmia, spanæmia, phthisis, &c.; 2, a variation in the normal blood supply to the brain; 3, a reflex irritation, though this may perhaps be classed under the second head, as irritation radiating from some distant organ would be likely to induce, by reflex action, contraction of the arteries; 4, shock."

It is conceivable that the first of these conditions may be a very efficient and potent element of causation, independently of structural change. This view, indeed, has been adopted by some very distinguished alienists as sufficient to account for the symptoms in acute mania, without the existence of structural change.[1] It is not so clear, however, that the second, third, and fourth conditions here laid down do not of themselves involve structural deviation from the normal state of the vessels of the brain. In fact, the lesions of texture afterwards recorded in this volume would seem to confirm this view. Dr. Fox states that Dr. Thompson, of the Bristol Borough Lunatic Asylum, has, by sphygmographic tracings in general paralysis, shown that this affection is marked in its very earliest stages by persistent spasm of the vessels. Extensive vascular changes are the most prominent of the pathological phenomena that are observed after death. The same may be said, as the author also points out, of mania and melancholia. The lesions of the vessels, nerve-tissue cells and neuroglia found in chronic mental maladies are all of such character as may be referable to a past inflammatory condition. The degenerations may in many instances be traceable to the changes incidental to old age, or to premature decay. The condition of the vessels and of the brain tissue has been described with great care by Dr. Batty Tuke, whose researches Dr. Fox has duly studied and appreciated.

Dr. Fox in the next place describes the changes that occur in the membranes; and after referring to the recorded observation upon deviations in the weight of the brain in various forms of insanity, presents a *résumé* of the various lesions that have been put upon record as met with in insanity.

From the preceding remarks our readers may be enabled to form an idea of the completeness with which Dr. Fox summarises our pathological knowledge with reference to the ner-

[1] This doctrine was ably expounded by Dr. Henry Monro, in a singularly elegantly written essay "On Insanity, its Nature and Treatment." 1851.

vous centres. The work that is being carried on will doubtless rapidly place in arrear the extent of knowledge conveyed in this excellent treatise, but will not supersede the value of the volume as a storehouse of facts and as a landmark for future investigators.

It is incumbent upon us to state that several other subjects are treated of in these lectures besides those already referred to, *e. g.* abnormalities, inflammation, and tumours; the pathology of delirium, of insanity, and of the many forms of paralysis, epilepsy, chorea, tetanus, hydrophobia, &c., receives consideration.

We must not omit to draw especial attention also to the graphic illustrations that Dr. Fox has himself executed, amounting to nineteen in number, which most faithfully represent the lesions they are intended to illustrate.

Every student of pathological science will do well to consult this comprehensive essay on nerve-pathology.

IV.—Carter on Mycetoma.[1]

A STUDY or desire to learn facts and the cultivation of habits of investigation in order not to overlook or despise little things in the natural processes of health and disease, are some of the characteristics of the age we live in; notwithstanding that there are fewer material rewards for the investigator in these than in other departments of life.

As might have been expected, well-directed scientific inquiry is constantly showing the fallacy of many views which were considered proven years gone by, and so in like manner our labours will, no doubt, in their turn be subjected to a similar process of refining or refuting by our successors.

Perhaps there is no subject of greater importance, and certainly none which occupies a higher place in thinking men's minds, or is fraught with more interests to humanity at the present time, than the relations which animal and vegetable organisms bear to the production and propagation of disease.

It is a subject regarding which there has been necessarily much controversy, which still continues, and leading observers advocate in many cases opposite views, so that we are unable with anything like absolute certainty to distinguish between cause and effect. Regarding the larger animal parasites the difficulty may not be so great as stated; but when microscopic organisms of a doubtful nature—animal or vegetable—are

[1] *On Mycetoma; or, the Fungus Foot of India.* By H. VANDYKE CARTER, M.D., Indian Army. London, 1874.

concerned, it is no easy matter to determine their exact value in disease, since it is well known that they sometimes are found to exist in the perfectly healthy body.

Consequently some microscopists and other observers have gone so far as to advance theories and make statements to the effect that the various diseases termed zymotic, including epidemic and many other diseases, are actually caused and propagated by fungi or bacteria, which multiply and produce effects rapidly ending even in a speedy death. For a period extending over more than fifty years, efforts have often been made to trace cholera in its various degrees of virulence to a fungoid origin, and of late Professor Hallier of Jena believed that he actually detected the fungus in cholera evacuations; but more recent investigators have shown that he was mistaken, for the fungus he has described and figured is not peculiar to cholera, but is to be met with in healthy evacuations in man and animals, as we ourselves have seen.

During the last thirty years Indian surgeons became acquainted with, and reported in the current medical journals, cases of a peculiar affection of the foot, which, though before that time known to native medical practitioners as keerenagrah (worm disease), tuberculous disease of the foot, &c., did not attract sufficient attention previous to the general introduction in India of European surgeons. We are indebted to Dr. Vandyke Carter, who was the first to discover a fungus in that disease, and to give a detailed description of the peculiar appearances and the microscopic anatomy of the disease, now generally known by the name of Mycetoma or fungus-foot of India. Dr. Carter's various communications on this subject in the Transactions of the Medical and Physical Society of Bombay, &c., with some additions, have been embodied in an elaborate monograph, which contains eleven well-executed chromo-lithographs, illustrating the external appearances of the disease affecting the hands and feet, and the general and microscopic anatomy of the diseased tissues. This work is a valuable contribution to the literature of the fungoid orign of disease, and the plates alone reflect great credit on the author as an artist, since some of them represent most accurately specimens which we have seen and are familiar with.

It will undoubtedly be the means of introducing the fungus disease to the notice of the profession, both at home and abroad, and will stimulate to further investigation, which is still much to be desired, in order to determine and corroborate many points stated by the author.

The name Mycetoma (from *Μυκης* a fungus) has been given to the disease, as the author believes that it is produced by the

entrance in some unknown way into the skin and tissues, and subsequent development and multiplication there, of the spores of a red mould of the genus Mucorini, and named by Dr. Berkeley, in honour of the discoverer, *Chionyphe Carteri.*

The natural habitat of this red mould of the genus Chionyphe, whether in the soil or in the water, or growing on plants, has yet to be determined, but it appears to us to resemble some forms of aquatic fungi which we have seen.

There are two varieties of the affection—melanoid and ochroid—to be distinguished, however, only when sinuses are formed in the foot or hand, discharging matters which are characteristic of each variety, and from which the red mould has been produced spontaneously and by cultivation.

It commences after a period of incubation not yet determined, in various ways, sometimes by a little, livid, localised induration of the skin without any pain or tenderness; at others by a vesicle, an abscess, or a boil in the sole, and the foot begins to swell, usually in a globular form from thickening of the tissues, and may attain a size measuring even twenty inches round the dorsum. Sinuses, often leading down to the bones, form in the sole and at various places, discharging profusely, and this condition may continue for years, until the patient's strength is exhausted, or until he is cured by amputation of the diseased mass. With reference to the reputed fungoid origin of the disease, it is very important to observe the time when the fungoid particles are first detected, or at what stage they appear, and accordingly at p. 14 it is stated, "Immediately consequent on the vesicles and abscesses, and sooner or later following the hard lump (which becomes softened at one part, and furnishes a soft bleb soon bursting), there appear sinuous openings, from which exudes a thin sanious or sero-purulent discharge, containing the characteristic fungus particles;" and again at p. 47, "Let me add that in order of time the fungi are to be seen at the moment when a sinuous opening is formed, and the latter seems to be produced for the purpose of giving exit to the particles." "The fact we have ourselves established, and the inference is a necessary one." If such be the case, and there is no reason to doubt the statement of such an accurate observer, then it is evident that the fungus must have existed, multiplied, and fructified in the tissues, previously to the formation of abscess or sinus, and it is more reasonable to believe that the spores or sporidia of such fungus at some previous period made their way into the tissues from without, rather than that they should be developed *de novo* from the tissues in the interior of the foot—a view which is not without its supporters.

When the disease is far advanced the whole foot becomes

disorganized and burrowed by membrane-lined canals, which usually intercommunicate, and contain in the melanoid variety black gritty particles varying from $\frac{1}{50}$ to $\frac{1}{10}$ of an inch in diameter, made up of fungi or their *débris*; while in the pale or ochroid variety the fungus-growths assume a pink, orange, or buff colour.

The appearances on section are thus described:—"A section of a much diseased foot presents at first sight an appearance of general confusion of parts; but this apparent disorder will be found upon attentive examination to resolve itself into some degree of method." "The pale or non-vascular appearance of the section is another marked feature; there is less vascularity than even in health." "The cavities in which are lodged the fungus particles are neither abscesses nor cysts; the membrane lining them and the canals is sometimes thin, but as often thick, velvety, pale, non-vascular, and friable; it is thrown into folds running longitudinally in the canals, but in the larger cavities (which are but lateral or terminal dilatations of these passages, and on section give rise to the semblance of cysts) the membrane is frequently arranged so as to impart to their inner surface a sacculated or even follicular aspect, owing to the formation by its foldings of numerous closely apposed secondary loculi, in each of which is lodged a separate accumulation of fungus particles; probably an even more intricate arrangement may obtain in the largest growths, but by patience the true disposition can always be made out."

Many persons in India and in England acquainted with the disease ascribe it to a constitutional origin, and consequently the author takes considerable trouble to prove by comparison with caries, elephantiasis, non-malignant tumours, &c., that it is a local affection, and in doing so we think that he advances strong arguments and has to a great extent succeeded to substantiate his opinion.

Judging from the general description given, it resembles caries perhaps more than any other disease; but the author states that it has neither the typical, local, nor common general characters of caries, and also that it possesses a special feature in its attendant discharge of small organic particles, which caries in Europe has never been known to present for notice.

The bones are usually affected either primarily or secondarily, and in a few cases myeloid cells were observed, but considered to be merely accidental products. The changes in the bones are described as peculiar and different from those of caries, &c., inasmuch as they contain small spherical cavities, surrounded by healthy bone, averaging from $\frac{1}{5}$ to $\frac{2}{3}$ of an inch in diameter, and communicate with one another and with the sinuses. The cavi-

ties contain fungoid masses of a globular form, which, by pressure on the surrounding healthy bone, produce the excavations. The joints, it is stated, are never primarily affected, but may become diseased from pressure of the foreign bodies.

The malady prevails chiefly among the agricultural population, in men more than women in a proportion of about 10 to 1, and is common from puberty to old age. It is strange that it has never been noticed in a European or Indo-European, though it affects the rich natives equally with the poor. Patients usually date the onset or commencement from a scratch, puncture, abrasion, hurt or prick of a spine of some prickly bush.

It is now many years since Carter's views were made public, and we are not aware of any inoculation experiments with the mould (*Chionyphe Carteri*) having been made on man or animals.

There could not be any grave objections to inoculate condemned criminals or animals with the spores of the mature plant, or with the fish-roe-like particles, or the pink particles or the black particles of the melanoid variety. In future investigations it would be well to observe if the disease spreads from one member of a family to another, or in hospital from one person to another, in other words, if it be contagious.

Nothing certain is known as to the manner of introduction of the spores, but Carter assumes it to be through the medium of hair follicles, or of the orifices of sebaceous glands and sweat-ducts, or else by abrasions of the surface. This notion is, however, purely hypothetical, for the spores have never yet been found in the above situations. The early symptoms of the disease most commonly commence in the sole, in which there are the orifices of the sweat-ducts, and of course the entrance of the spores in that region is quite possible; but we think that the discharge from the ducts would tend rather to prevent them from entering.

The author shows that cases of mycetoma, if left to nature, proceed from bad to worse until the patient dies from exhaustion or the supervention of some induced disease; therefore the prognosis is invariably bad.

A few pages are devoted to treatment and preventive measures, and as regards the latter he recommends the wearing of shoes, &c., "and the daily application of wood-ashes or tarry liquids to the feet during the rains," probably with the object of preventing the entrance of the fungoid spores; and we can fancy the difficulty and novelty of daily tarring the soles of many millions of Her Majesty's subjects, since the affection is "very widely spread in India."

In proof of the local nature of the disease he points also to the effects of treatment and the relatively small rate of mortality after operation (only a little more than four per cent.), and shows

that cases treated by amputation beyond the affected parts seldom or never recur—results which in a constitutional disease would be different. The same success can be attained in incipient forms by the use of other measures, such as excisions of the affected skin and soft parts, nitric acid, potassa fusa, the actual cautery, &c., and cases illustrating the beneficial effects of the various methods of treatment adopted by different surgeons are recorded. In ordinary fungus diseases of the skin, tonics and general treatment are beneficial, but in mycetoma they are unattended with decided benefit—another proof, the author thinks, of its local and parasitic nature. There appears to have been no trials made of the effects of injecting into the sinuses the various reputed chemical solutions which are extensively employed locally in diseases of supposed parasitic origin, but the subject is referred to as worthy the attention of future investigators.

A minute description of the hard black particles of the melanoid variety is given at length. These were formerly considered to be collections of fungoid structures, but are now held to be "sclerotia," composed, as in the common mildew and other moulds, of mycelium filaments. He shows by a series of figures in Plate IX that the masses are made up of fungi, but admits at page 72 that there may be varieties of the sclerotioid particles in which the fungus structure is lost or destroyed, so that an ordinary observer examining such specimens might reasonably infer that a fungus structure never existed in them. Would such an explanation serve to show why other observers have not been successful in detecting a fungus in cases of Mycetoma? The effects of reagents on these bodies are slight; acetic acid, the alkalies, and ether have but little influence on them, while iodine, it is stated, produces a brown tint, distinctive of cellulose, and hence it is inferred that the particles are of a true vegetable nature.

In the ochroid or pale variety of the disease the fish-roe-like masses are termed malacotia, and "the fungoid elements assume the character of pink, light brown, yellowish, or pale buff particles of minute size and soft consistence; their number is immense, and their prevalence is universal in the affected foot."

In a specimen of a foot with madura disease, the only one of its kind ever seen, the new material was of a general pinkish hue, and under a high magnifying power appeared to be formed of round, ovoid, quadrate, dumb-bell shaped, double or even quadruple-shaped bodies, the figures of such being quite different from any structures we have been accustomed to examine, and what their pathological significance may be is a matter of conjecture.

The soft particles generally met with are composed of a cen-

tral body from $\frac{1}{50}$ to $\frac{1}{300}$ of an inch in diameter, and surrounded by colourless crystalline spiculæ not freely acted on by reagents. The author says, page 76: "This indifference of these pale, soft particles to chemical influence is to me strong evidence of their vegetable rather than animal nature, and the occasional absence of amyloid material, &c., is no objection whatever to the conclusion which is here drawn."

This view of the composition of these bodies has been disputed by other microscopists, who could detect no trace of fungus after the application of various reagents; but it is stated, nevertheless, that from one of such particles after cultivation in rice, the growth of *Chionyphe* was observed by the author.

In some sections of the diseased parts in both varieties, pink coloured streaks were observed in various situations and adjacent to the canals, and were supposed to indicate the commencement of the fungoid growth by ante-inoculation from pre-existing spores in the sinuses. The pink deposit referred to is composed of minute bright-tinted and clear globules from $\frac{1}{6000}$ to $\frac{1}{4000}$ of an inch in diameter, and their connection with bacteria is conjectured. "In short," Dr. Carter writes, "the first known trace of the entophytic growth occurs under a form seemingly referable to a bacteroid origin," and this is, no doubt, an inference from the fact that "according to the opinions of competent observers from the spore of a fungus may directly proceed bacteroid bodies, and that as a normal process." (Hallier, Huxley, &c.)

This appears to us a very poor guess, but the author goes further, and traces a resemblance in structure between the pale particles of mycetoma, the micrococcus-colonies of Hallier, the bacteria colonies of Klebs, and a peach-coloured bacterium described by Mr. E. R. Lankester as occurring in putrescent water. In a note, page 83, he writes: "It is, I may add, satisfactory to myself to have at last found the type of structure to which may, to all appearance, correctly be referred these hitherto anomalous bodies found in the human disease," and again at page 84, "Once for all, I would state that the presence of a *Bacterium*, whether moving or motionless in connection with the early (or perhaps any) stage of Mycetoma has not been demonstrated." No assistance will be rendered in future investigations into the pathology of the disease by reckless speculation or by stating that "the soil of both field and lane in India so invariably swarms with decaying vegetable matter, and this at certain seasons in a moist condition, that one can only wonder that the barefooted native does not far oftener become affected with the fungus disease."

The Rev. Dr. Berkeley hazards the conjecture (page 93) that the disease might be produced by the spores of *Saprolegnia* in

stagnant pools. Saprolegnia is an aquatic fungus found growing parasitically on the bodies of dead flies, lying in water, also on fish, frogs, &c., and occasionally on decaying plants.

A specimen of diseased foot (Carter's pale or ochroid variety) was exhibited by Dr. Tilbury Fox before a meeting of the Pathological Society of London in 1869, and the Report of the Committee appointed to examine it was to the effect that "there was no trace of structure that could be set down as that of fungus." In 1871 a specimen of the melanoid variety was exhibited before the same society, and reported on by Dr. Bristowe, who found fungoid structures abundant; but he could not "decide whether the fungus is the cause of the bone disease or only a consequence of it."

Dr. Fayrer, of the Indian army, has recently published a work entitled "Clinical and Pathological Observations in India," and at page 637 reference is made to a case of Madura foot. It was carefully examined microscopically by himself, Professor Ewart, and Dr. Timothy Lewis without finding anything to support the theory that the disease is due to the presence of a fungus. Fayrer, who has had a great deal of experience in Indian diseases, inclines to the belief "that the fungoid growths, when they do occur, may be aconsequence and not a cause of the disease, whose affinities are more probably to be referred to Elephantiasis or some allied constitutional disorder than to the presence of a vegetable growth possessing the strange and seemingly improbable power of finding its way into the depths of the tissues forming the foot."

Dr. T. Lewis, of the Army Medical Department, well known for his investigations in India, endeavoured to cultivate the fungus by adding thirteen specimens of the material forming the diseased patches in the case above quoted, to thirteen various cultivating media or their combinations placed in moist chambers. The specimens were watched from the 14th of August to the 7th of November, 1871, and so far as the appearance of any peculiar fungus was concerned, the results were negative. On the other hand, Dr. Carter accounts for the failure of these cultivation experiments by the lateness of the season at which they were undertaken, and on this point writes that "the new growth appeared only at a certain time of the year, which for Bombay may be regarded (other facts being corroborative) as the close of the hot season and setting in of the rains, or from April to July or August."

In the February number of the 'Indian Medical Gazette,' there is an article on "The Etiology of the Madura Foot," and though the name of the writer is not given, it has evidently been written by one possessing a knowledge of fungi in general

and of this subject in particular. Any accurate observations by a person as it were present on the spot where the disease is endemic should have due consideration, and this observer, no doubt from a study of the disease, differs from the conclusions arrived at by Dr. Carter, or, in other words, does not admit as proven that the affection is produced by a fungus.

Dr. Carter hints at some relation between fungus foot and guinea-worm disease, and it is difficult to comprehend what that relation is; but it certainly seems strange that the spores of a fungus should enter the integument, penetrate the solid tissues, and form canals on something like a definite plan and sometimes in situations which give the least resistance.

This subject is well worth investigation both with regard to the other points we have noted and to many others. As we have seen, there is above all no unanimity regarding the etiology of the disease.

The book contains a good deal of repetition, possibly from a desire of the author to impress his views deeply on the minds of his readers; nevertheless we recommend it as a most valuable and careful record of investigations into the nature of a disease still surrounded with a great deal of mystery.

V.—Horton on Diseases of the Tropics.[1]

It is now fifteen years since the last work of any size or pretensions which treated generally of the diseases of the tropics appeared in England. Some small works on the diseases of India by Peete, Moore, and others, have come out since, but do not seem to be extensively known, and the second edition of Morehead's clinical researches continues to be our best and most modern work. Not that there has been any want of labourers in the field of Indian medicines. Detached papers in Indian journals, and admirable official reports, such as those of Bryden, Cunningham, Cornish, Lewis, and Cunningham, are abundant. Indeed, it may be a question whether these public documents do not to a certain degree interfere with the labours of individuals. We have had of late years a few articles and books of some consideration on Indian medicine, as Gordon, Macpherson, Macnamara on cholera, Chevers on public health, and Oldham's ingenious discussion on malaria. But we have not been supplied with any general view of the present state of

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[1] *The Disease of Tropical Climates and their Treatment, with Hints for the Preservation of Health in the Tropics.* By J. A. B. HORTON, M.D. Edin., F.R.G.S., Surgeon-Major. London, 1874. Small 8vo, pp. 657.

Indian medicine, and especially with no work that brings into one focus all our most recent knowledge respecting the fevers of India, and endeavours to determine their relations to each other. In former days there was no lack of such works, many of them bearing the impress of the individual character of their authors. Lind and Clarke were followed by James Johnson, Twining, Annesley, Gedder, Martin, Macgregor, Mackinnon, and the series seems to have wound up with Morehead. In like manner for the West Indies, besides the various works on the diseases of sailors, we had Hunter, Bell, Rollo, Manby, Chesterton, and others. But of late years, with the exception of the reports on yellow fever, and Milroy's late report on leprosy, we have little but what is to be found in the official reports of the army and navy. The French have contributed good material in their accounts of the semi-tropical climate of Algiers and of parts of Africa; and Boudin and Armand have illustrated the geography of tropical disease. Some American writings on the climates of their southern States are also of value. Still we are not aware that any country has furnished a good manual of tropical disease. Every one complains of the non-existence of such a work, and a laudable desire to supply the want has induced Dr. Horton to come forward. It was a bold task for him to undertake, living in a situation of comparative isolation on the western coast of Africa, and away from libraries, but he has performed it with very creditable success. It was scarcely to be expected that he should be able to produce a completely satisfactory work, embracing an outline of the whole subject. He seems to have consulted all the authorities he could, and he mainly rests on the labours of others; he especially follows Martin, whose work having grown out of Johnson's older one, was never quite up to the knowledge or practice of the day, and we see no evidence of his being really acquainted with the writings of Morehead, although he makes reference to them when speaking of the use of ipecacuanha. We think Dr. Horton would have been better occupied in giving an account of his personal experience of the diseases prevalent on the coast of Africa. All works giving a faithful picture of actual observation are valuable, but the present book is singularly deficient in the local information which he must be well able to supply. It is not necessary to enter into a lengthened review of a work which handles so many subjects, but we shall glance at a few points which have struck us. We could easily take exception to the style, which is often ungrammatical and obscure. We do not quite understand his selection of subjects; why, for instance, dyspepsia, constipation, and rheumatism should be specially selected for discussion in a work on tropical diseases.

One of the best chapters is on splenic enlargement, and its occurrence as a result of endemic influences, or of simple chronic malaria poisoning independently of any accession of fever. This, we believe, is of more frequent occurrence than is usually supposed, and is developed chiefly, as our author remarks, in persons of mixed blood, and in those who are born in the tropics. He states that occasionally infants are born with enlarged spleens like their parents. We hope that he has entirely satisfied himself as to the accuracy of this. It is difficult to know what Dr. Horton thinks on the subject of dracunculus in infants; he says, "one is sometimes told," that it attacks children a few months or weeks after birth. He does not say that no such case is within his own personal knowledge: but that the worm could not be transmitted through the mother, as the embryo worms are too large to make that possible. Then, again, in the case of Guinea-worm in the scrotum, he ascribes this to direct local contact with an infected person, which is surely an improbable cause. The chapter on ardent continued fevers is one of the least satisfactory. Febris perniciosa, malignant or typhoid fever of the tropics, are given as synonymous terms. Now, if by febris perniciosa is meant the *fièvre pernicieuse* of the French, that is an aggravated remittent or intermittent, which is something entirely different; and as to typhoid fever of the tropics, although he does mention ulceration of Peyer's patches, surely he does not describe enteric fever. However, this is the only notice he gives of that disease, and perhaps after all he does allude to it, as he says that its true cause is putrefying animal matter, and the contamination of drinking water by it. His treatment, which consists of 1 grain calomel, 2 grains of quinine, and $\frac{1}{2}$ grain of opium every hour, is remarkable, but does not throw much light on the question of what sort of fever he had in view.

Dr. Horton often lays down the causation of disease in a somewhat positive manner, which, however, is a not very uncommon practice at the present day. He tells us that "when the system is much surcharged with miasmatic effluvia, it (what?) makes its escape through the intestine by causing irritation in its coats; this form of disease is called malarial diarrhœa." He informs us that when there is much ozone in the air, there is less malarious fever; and also that when this is the case, smaller doses of quinine are sufficient to cure it: interesting facts, no doubt, if they have been duly ascertained.

A more important statement, and one opposed to the ordinary belief, is that it is a mistake to suppose that Africans, and especially that West Indian blacks, enjoy any immunity from malarious fevers. Dr. Horton says that in dysentery "evacua-

tion is mostly in lumps or hardened balls resembling forced-meat-balls, but harder, but they are by no means frequent." Does he mean that these hardened balls or the evacuations are rare?

The accounts of endemic diseases are on the whole poor, that of dracunculus the best. The remarks on treatment are generally judicious. Dr. Horton does not care for very large doses of quinine, and thinks that eight-grain doses will answer almost any purpose. He believes in the usefulness of quinine taken in small doses as a prophylactic against fever—a matter which is very questionable. He speaks nearly as sanguinely as others of late have done, of the specific effect of ipecacuanha in dysentery, although he has occasionally observed alarming effects, he thinks, from too long continued doses.

It is wonderful what vicissitudes of unlimited employment and of nearly as universal disuse in dysentery, that root has gone through. Dr. Cockburn wrote in 1736, "We were religious adorers of a specific, the ipecacuanha, as well as the French for half a century, but the French have been cured of their credulity for some years." Its first employment was nearly as early, at least as early known to us, as bel, the remedy for chronic diarrhœa. We are able to confirm, from personal experience, what is said of the valuable effects of sugar of lead in some cases of dysentery, especially in natives of the tropics. We think Dr. Horton need not dread the production of symptoms of lead poisoning. He does not seem to have heard of the use of large enemata in that disease—a practice for a time in vogue in India, on official recommendation, but one, we believe, now nearly forgotten. He maintains a judicious reserve as to almost miraculous effects in spleen and in goitre, ascribed by very competent authorities of late years to the local application of ointment of iodide of mercury.

Of his remarks on tropical hygiene we shall only say, that the maxims culled from various sources are sensible enough, but many are redundant, and clothed in such queer language, that they almost seem intended to cast ridicule on the subject. For instance, "A strict moral principle is beneficial in the tropics:" "The Christian religion has a more beneficial influence on the minds of men in the tropics than either Mohammedanism or any other religious tenets."

It would be easy to point out many such strange and barely intelligible statements. Nevertheless, with all its defects, the book is one that is not without its use to medical men visiting the tropics for the first time.

Instead of following Mr. Horton further, as we have said that a book on tropical diseases is a desideratum, it may be better

to sketch our notion of the sort of treatise that is wanted, and of the subjects which it should comprise. Of all the works that we have seen, Morehead's is the most satisfactory, and a new edition of it, with certain modifications and additions, would be most valuable, but a little known book was published in India in the year 1848 by the late Dr. K. Mackinnon, which in its scheme embraced nearly every subject which should find a place in a work such as we contemplate, although its execution was not equal to its design. Indeed, such a subject cannot be treated adequately in a book no larger than his or than Dr. Horton's manual. When we talk of a manual being urgently wanted, it is not so much that there are many important novelties, either in the pathology or the etiology of tropical diseases, which have only been made out of late years. But in all sciences we like to have new "presentments" of the subject. Even if the matter is chiefly old, still we like to have it, like our coats, shaped according to the fashion of the day.

We have already alluded to some of the chief sources from which aid in the construction of such a work should be derived. It is to be hoped that ample references will be given to those sources—a want of such references makes us often entirely forget that the ground we are going over has already been trodden by other observers.

A writer who shall put together a satisfactory work, must be able to combine in his own person both a knowledge of the opinions of others, and a practical familiarity with disease.

Many sanitary reports, both in Europe and in the tropics, are drawn up (and in a very positive and confident tone also) by men of little actual experience of disease. Many popular manuals have been produced by writers who knew little or nothing of the bedside. Useful though such works may be, the practitioner on referring to them cannot fail to observe a certain want of practical knowledge, which detracts greatly from their value. The latest microscopical researches, and the most recent modes of treatment are too often advanced in an unqualified way as matters entirely beyond question, instead of being received and considered in a judicial frame of mind. This leads us to remark, in passing, that the two Indian microscopists, Drs. Lewis and Cuningham, distinguish themselves from many of their brethren in Europe, by the caution with which they avoid hasty generalisations. A work of the kind we speak of, might easily be made to include whatever was special to the case of sailors or of coolie emigrants, or to the condition of soldiers, and no doubt would be welcomed by army and navy surgeons, as well as by those of our mercantile

marine. It is comparatively an easy thing to plan such a work —to execute it would be a matter of difficulty and of labour. Different modes of arranging the material will suggest themselves to different minds, but a work of the sort we speak of, might treat of the following subjects.

The differences between climates temperate and tropical, extremes of variation of diurnal temperature; of atmospheric pressure, amounts of rainfall and of moisture in the air, of what is termed malaria; the effects of seasons on the prevalence of particular diseases; a slight sketch of the geography of disease.

Hygiene of the tropics; hints on food and drink and dress; improvement in the value of European, query, of native life (on which there has been much interesting discussion lately); effects of change of place, everywhere an important remedy, but especially so in the tropics. This would include some account of the effects of diminished and increased atmospheric pressure on the system, of mountain climates, and of hill-stations.

Endemic diseases might be more fully alluded to than has usually been the case. They have never hitherto been treated of with the completeness that might have been expected. If we now enumerate chiefly diseases of the natives of the East Indies, it is because we happen to be more familiar with them. Some affections of the digestive organs, as dry belly-ache, dirt eating, colic, aggravated dyspepsia as *pet sool;* of respiratory and circulatory organs, as epidemic catarrh or *nacra*, affections of lungs; prevalence of aneurism and of nervous diseases, as tetanus, especially in women and new-born infants; liability to paralysis from early excess, from cold; local anæthesia, burning of the feet; insanity induced by drugs; sleeping disease of Africa; nyctalopia, amaurosis, spleen, land scurvy, beri-beri, leprosy, elephantiasis Arabum, endemic hydrocele, syphilis.

Cutaneous affections, boils, cracks in soles of feet, calculus, chylous urine, bloody urine, goitre, and cretinism.

Helminthiasis, dracunculus, generation of maggots, bites and stings, of mosquitoes, sand flies, chique, serpents, jackals, dogs, hydrophobia.

Other divisions of the subject may be more briefly alluded to, as they are obvious to every one. What are called ordinary tropical diseases are:

Fevers and their varieties, *coup de soleil*, cholera, diarrhœa, dysentery, liver diseases and their effects, chronic diarrhœa, visceral enlargements, neuralgia, local paralysis, and anæmic conditions. These might be treated of, as they are influenced by hill-climates, sea-voyages, and residence in Europe. On the influence of European climate Sir R. Martin's book is of value, and

we might hope to have the experience of the hospital at Netley and especially of Professor Maclean. Affections with which we are most familiar in Europe should likewise be treated of as they are modified by tropical climate,—eruptive fevers, scrofula, phthisis, heart and lung affections, diseases of the kidneys, rheumatic gout, hæmorrhoids, delirium tremens, and syphilis. Morehead has worked out portions of this division.

Additional chapters, too, are wanted on the diseases of women, and the influence of tropical life on their systems; and on the diseases of children. In these last the mildness of eruptive fevers is counterbalanced by the severity of the affections of the alimentary canal. To fill in the sketch we have made, would necessarily be a work which could not be executed well, unless it were written with due deliberation.

But as we have already remarked, our most pressing want is an account of fevers as they occur in the tropics, a consistent history of their varieties. Besides differences of opinion about yellow fever, there are diverse ideas regarding enteric fever in India, regarding the nature of the devastating Burdwan fever, and the gaol fever of the Punjab. A good account of the fevers of the tropics might be furnished with comparative ease by some of those who are now labouring in the field of tropical medicine.

While this is passing through the press, we observe that several French works have appeared on tropical climatology and disease.

VI.—Heredity.[1]

Is hereditary taint, tendency, and transmission an established and trustworthy truth, and, if so, what are the elements handed down from generation to generation? The present seems to be a suitable stage in the progress of science to attempt to discover an answer to this question, when the subject has assumed such vast proportions and is ever widening its range, when it possesses a voluminous and ever-increasing literature of its own, when we have introduced Gallicisms into our language in order to give greater precision to the inquiry, and when the inheritance of forms and qualities is employed as a universal solvent for physiological and pathological difficulties otherwise irremovable, and, we suspect, for many such difficulties as are to a

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[1] 1. *Heredity, a Psychological Study of its Phenomena, Laws, Causes and Consequences.* From the French of TH. RIBOT, author of *Contemporary English Psychology.* London, 1875. Pp. 393.

2. *Heredity and Hybridism.* A suggestion by EDWARD S. COX, S.L., author of *What am I?* &c., &c. London, 1875. Pp. 66.

3. *Hereditary Genius, an Enquiry into its Laws and Consequences.* By FRANCIS GALTON, F.R.S., &c. London, 1869. Pp. 383.

certain class of minds altogether insoluble. The all but universal assent with which the dogma of heredity is accepted by the learned and unlearned alike is referable, we conceive, to the observation of the similarity in form, feature, and character, in families and races—in fact, to the recognition of what constitutes species—when science was not, to the equally early perpetuation of castes, occupations, trades; to the popular craving for wonder-working influences, and, above all, to the sanction afforded by the scattered memorabilia collected by mediæval physicians. Since the age, however, when common Fevers, even a Quartan Ague, were supposed to be transmissible from mother to child, and since that time, when Haller recorded that the Bentivoglios had a slight tumour on the body, and when superstition or fancy, we hope, added that this nævus warned them of changes in the weather, and increased in size before the approach of moisture, the province conceded to heredity has been so enormously enlarged as to embrace all diseases, dispositions, depravities, peculiarities in form, function, even some acquired habitudes and oddities, descending from the noblest attributes of Reason to intolerance of the touch of metals (as exemplified in an existing noble family), and has been elevated into the rank of a symptom or the substitute for less palpable symptoms in disease, and in the instances of insanity and responsibility to take the place of better appreciated and more justifiable grounds for depriving the victim of his personal liberty and social rights. The heterogeneity of the various internal and external marks of defect or redundancy, traced in large families and long lineages, might be legitimately urged as a valid objection to the whole theory, but is here introduced merely to indicate the lack of simplicity and the data upon which it is founded, and in order to show that there must be a multiplicity of factors at work, and totally different laws in operation in the process of transmission, should such a process be found actually to exist. It will now be seen that the terms of the question initiating these remarks were advisedly selected, and that the descent or diffusion of syphilis cannot be identified with a tendency to drink, steal, or to seek and obtain distinction in oratory or music, and that neither can have any affinity with the reappearance of muscular strength and rickets, of personal beauty and elegance and deformity, in a long line of relatives. How clumsily such morbid genealogies are sometimes constructed may be illustrated by that of the Cæsars, which is generally advanced as a *cheval de bataille* in such encounters. In this stream, supposed to be so continuous, it falls to be noted that the mother of the great Cæsar was no ordinary woman, that his daughter, who married Pompey, was famed for feminine wit and beauty; that the heritage of these rulers of the world

appears first in talents so great and varied that they would have distinguished either a statesman or a soldier, but associated with epilepsy; next in Augustus as valetudinarianism; then in Tiberius as ichorous disease of the skin and face; fourth, in Caligula, as pallidity, pervigilium, and delirium; fifth, in Claudius, as physical imbecility; and, lastly, it is consummated in Nero, as unequivocal insanity. The enumeration is not cited as a *reductio ad absurdum*, but as embodying many of the vices and defects detectable in the attempt to demonstrate the course and features of Heredity. Here there are, first, a fountain of intellectual power and bodily perfection, but giving forth turbid and polluted springs, which have not only lost their original purity, but are gradually poisoned by additions, which appear to have little connection with each other, and can scarcely be regarded as showing a common origin, or even a similarity, between the different causes from which they may have proceeded. By an extreme stretch of imagination we may admit that the epilepsy of Cæsar and the madness of Nero were of kindred root, but no recourse to ingenuity or to the hypothesis of atavism could invent a link between the valetudinarianism of Augustus, the skin disease of Tiberius, and the physical weakness of Claudius. In fact, history tells little more than that this family were of diseased constitution, and remarkable by their power, position, mental strength, or moral weakness. The practice of endeavouring to establish a law of heredity by pointing out the liability of certain families or communities to certain classes, though not to the same class of diseases, is prevalent in the present day, and we have elaborate treatises and tables to prove that scrofula, phthisis, gout, rheumatism, convulsive affections, and insanity frequently affect members of the same family or their collateral connections, with the implied or appended inference that they are congeneric affections, and, though separated so widely by the tissues involved, and the nature and event of the maladies themselves, they may be traced to a common factor in hereditary tendency. To this generalisation the character of the latter member of the series, insanity, although believed by many to have its seat in an immaterial principle, and by many more to be influenced and cured exclusively by moral agents, seems to present no insurmountable obstacles to the authors of these speculations. To mental diseases the advocates of the doctrine of the propagation of psychical contamination have constant recourse, as affording abundant and inexpugnable evidence upon the subject. This step is natural and justifiable, as, apart from the publicity given by gaps in the social circle and the misfortunes they entail when patients thus afflicted are consigned to hospitals, where the

cause and concomitant circumstances of their malady are recorded, their antecedents may be approximately ascertained. But, in examining such cases, and running over the items founded upon such histories, and finding there three or four generations and several individuals belonging to each generation, it is impossible to avoid the conclusion that, in order to admit even a modified heredity, we must take these items in the lump as shadowing forth the proneness, the susceptibility of certain consanguine groups to cerebral disturbance, but not to the same kind or phase of mental perversion. We saw, upon one occasion in the same institution, a mother, her son, and daughter, and the daughter of the latter, who were all insane; but, on investigation, it appeared that the mother was a composed, though garrulous, incendiary, who set fire to her cottage when it contained all her children; that the son was a coarse, drunken, delirious person, of limited capacity; that the daughter was a violent, destructive, paroxysmal maniac; while her daughter was a weak-minded melancholic; so that, though connected by blood and a common lot, there was no other relation between their several conditions than that of mental disorder. Moreau de Tours has estimated that of lunatics nine tenths are hereditarily affected. Other authorities make the proportion one tenth; but M. Ribot calculates that one half or one third owe the origin of the disease to this cause. But, if this gross number be analysed, it will become obvious that the predisposition to lypemania, or to other very distinct species of depression, may have been sanguinary and impulsive mania; that the vain monomaniac or proud general paralytic may have had a progenitor who committed suicide, and that individuals labouring under confirmed dementia have had among their ancestry odd or excitable and intemperate notabilities. That many instances of dipsomania and drunkenness may occur in the same family, may eventuate in lunacy or even structural disease of the nervous system, and may recur in descendants, cannot be doubted; but, unless the whole postulate, that the required habits and vices are inherited, and unless the effects of imitation, similarity of disposition, and positive training, be ignored, it will be difficult to trace out satisfactorily such a heritage; but the most striking feature in the supposed influence of drunkenness over the moral constitution of progeny is contained in the proposition first enunciated by Howe, now received almost as an axiom, that more than one third of the idiots in America are the offspring of intemperate habits. In this country, although examples of such causation may be met with, their frequency is not so great, nor would it be philosophical to dissociate this cause from the general dissoluteness, the disease, and the

poverty, which may exercise a greater influence over such a result; but, even if idiocy and imbecility could invariably be connected with habitual intoxication on the part of a father, it is difficult to see why the temporary saturation of the brain with alcohol, on the one side, should lead, by any law of heredity, to the non-development of intellect upon the other; it is difficult to see why the brief obscuration of mind in the propagator should almost always lead to structural deformity and imperfection in the propagated, and still more difficult to concede that, if the law of heredity be inflexible, as is represented, the stalwart frame of the father, as well as his brief obscuration of intellect, should not have been imparted contemporaneously to the child. But in many respects this law is neither rigid nor regular in its application. Confining our present view to pathological considerations, it is notorious that the morbid taint often passes over a generation, that perfectly healthy and robust individuals may be the only connecting media between others suffering from gout or consumption; and, in order to obviate this apparent contradiction, the theory of atavism, the recognition of which is attributed to Haller, has been propounded. While it is inconsistent with even what is generally admitted as to heredity, to affirm that a pure and unvitiated stock should produce a corrupt seed, the obstacles to a clear comprehension of the relations of these links in the chain become insuperable when several generations have been free from disease, when the proclivities of collaterals have to be taken into account, when, in the search for some peculiar or poisoned source, we are carried back for scores—perhaps a century—of years, during which the stream of life has been flowing apparently without contamination or marked deviation from its natural course. But this artificial expedient of reversion is found to be inadequate to connect the phenomena, and cross or intercurrent between the sexes, collateral, and even avuncular heredity is resorted to, which M. Piorry, though viewing with suspicion, and ignoring altogether the effects of old and strong over young and feeble minds, of punishment, praise, and of the instruction, so often usurped by uncles and aunts, conceives to be probable. But this difficulty is obviated by another hypothesis, which, with all deference to those who hold it, may be styled untenable and undemonstrable. In order to bridge over the gap between the persons affected with disease, whether that gap include one or many undiseased persons, it is asserted that the taint is present in all, but lies latent, dormant, inactive, but is infallibly ultimately called forth. We are much tempted to quote, as appropriate, "De non apparentibus et existentibus, &c.," but shall content ourselves with suggesting the rashness of arguing upon latent, that is, non-appreciable, qualities, upon taints which have

no symptoms, upon tendencies which tend to nothing in the individual, whether they affect the species or not, and with the formidable commentary that this latent germ does not bud nor bear fruit until *called forth*—that is to say, until vivified by surroundings which are in themselves perfectly equivalent to the production of the effect attributed to heredity. Were the doctrines of atavism and latency pushed to extremity, in place of pointing to exceptionally degenerate races, we would fail to discover a single clan, tribe, family, exempt from the causes of such deterioration, or in which the tenure of life and sanity was not precarious.

Let it be supposed that the *fact* of latency were admitted, it is next incumbent to determine what the thing, the undeveloped germ which passes through a succession of living beings, but appears in certain members only of the series, whether it be a faculty or force, a primordial form or fantasm, a poison or a panacea; or whether we must rest content with the vague generalisation of a beatific or morbific proclivity, slumbering far down or awaking as the case may be, in the depths of structure or sensibility. But the perplexity is augmented when it is considered that many such proclivities must run through the same channel, sometimes connate, sometimes antagonistic, sometimes mutually destructive. Authors alive to the importance of these views have attempted various explanations. In reference to the transmission of mental powers or perversions, it has been held to depend upon the form of the head, the quantity or quality, or the proportions of the cerebral substances, but this dependence of the issue upon the natural or malformed cerebrum where distinguished talents or irregularities of conduct appear as characteristic manifestations, or where imbecility occurs, or where age or circumstances press too heavily upon some weak or wounded part and the whole fabric falls; was obviously begging the whole question. Again, it was argued that insanity and all the allied deviations of eccentricity, extravagance, weakness, might be propagated as specific diseases, depending upon an unhealthy condition of the brain or of some other organ acting dynamically, which was roused into activity by the slightest causes, and may burst forth into disease from the acceleration of the circulation in fever or passion, or from the influence of internal emotions or external events. But, as such causes directly produce similar consequences, there does not appear to be any necessity for speculating as to latency, but, where these consequences do not follow, such exemption may generally be referred to one of three sources: 1. Either one of the parents has been of robust and pure constitution. 2. The natural physical powers and training of the individual exempt have been such as to afford protection; or, 3. It is rank or

position, or profession in society, has removed him from the influence of the excitement, fluctuations, and reverses of fortune which are so fertile of danger. In scrofula, gout, lepra, syphilis, a distinct virus has been supposed to be the medium of communication, where particular features, departures from ordinary conformation, or actual deformities, appear in families, a change of type, effected by external or internal influences, of which we know nothing, has been believed to take place. But, if deficiency or redundancy of fingers or toes, distortions of the limbs, ears, nose, be produced, be acquired by any external cause acting upon the structures of one individual, it will become necessary to show why circumcision, tatooing, disfigurement of the cranium, as practised in certain nations, have not become permanent forms transmissible by heredity. Where no such flagrant deviations from nature either took place or were to be expected, a *taint*, an infection, an unhealthy element, was suspected of interpenetrating the system, which cannot have been other than disease in its first stage; which cannot explain the development of new diseases such as smallpox, syphilis, &c., dependent upon social customs or corruptions, nor the origin of epidemics or contagious diseases. Again, a *tendency* or predisposition has been conjectured to exist which, if it means anything more than "as the twig is bent the tree's inclined," must express the fact that certain individuals from defects in structure or strength, from certain pursuits, propensities, &c., are more exposed to external circumstances than others. Again, a *law* is imagined to be impressed "in our members," which cannot be otherwise understood than as that form of organization and those functions which constitute the species to which we belong. But still more transcendental solutions have been essayed; certain of these have been sought in chemistry, certain in mathematics; and Serjeant Cox has sought refuge in anatomy. The learned serjeant conceives that reproduction is not the initiation of a new, but the iteration of an old life; that while there is a homogeneous germ of life common to all organised beings conferring the type of species, the character and form are eliminated from this by nerve influence. But even were it conceded that the ovum is not merely a congeries of molecules, but a force, we must demur, as Wollaston demurred, to the notion that the Soul, or even "a manifestation of soul," is thus transmitted. Serjeant Cox goes on to say that considering the simplicity of nature it would be a fallacy to attribute to one parent what must be due to both, and regards the more recent as the more rational theory, that the father supplied the germ, and the mother was the nurse. The twofold is not duplex, as a sculptor would have made it, but two halves. These two

germs, which are undeveloped nerve-centres, are attracted to each other by a force unknown to us, and thus acquire the material necessary for unity. Upon such germ, which is defined to be a cell, not a protoplasm, are stamped life, shape, qualities, the type not merely of the species, but of the individual animal's progenitors, which are not obliterated when the minute creature grows by expansion. It is not regarded as an exact fac-simile of the parents, but as in conformity with their original and developed nervous systems. Upon this compound development appears to hinge the learned serjeant's doctrine of heredity, the interfusion of the maturer or developed qualities of the respective factors preserving uniformity, but preventing multiplicity in the characteristics handed down to the offspring. This duplicity in union is advanced to elucidate the existence of double organs, symmetry, the occurrence of twins, of monstrosities, &c., and it might have been pushed as an explication of the contrariety of disposition in the same mind; of the dissimilarity of the two sides of the body, the one resembling the father, the other the mother, and of Dr. Wigan's notion as to the independent action of the two hemispheres of the brain. Dr. Coxe imagines that this likeness to parents is more detectable in the mental than the bodily qualities, and that the cropping out of such traits in the descendants is the proof of the original hybridism.

Had there been perfect stationariness and immutability in the operation of innèité and heredity, it is quite clear that we must have remained the exact representatives or simulacra of the primeval inhabitants of the earth; that, notwithstanding the omnipotence of evolution, the transition and the steps by which the passage was made from the bone to the stone, from the stone to the copper, &c., ages, would become incomprehensible, that the influence of external impressions and influences would have been nullified, that the efforts of genius and discovery could have but feebly elevated the destiny of our race, and that this institution of natural primogeniture would have bequeathed to us an enslavement of fatalism unheard of in ethical or religious systems. Of course such an outcome does not prove that the premises are destitute of validity, and it is prudent to trace out the process which, although binding nature "fast in fate," may have "left free the human will." It is confidently asserted that heredity is the original factor of instinct and psychological development; although, unless there be growth, addition from one point to another, that is a departure from and in the element transmitted, it is impossible to grasp the origination here indicated. If the mechanism upon which heredity depends explains the rise of the lower instincts, it may likewise explain the development of higher psychical conditions; but if it fails in the former

it must fail in the latter. The next assumption is that instinct is an unconscious form of intelligence determined by organisation, in other words, a product of cerebral matter, underlying what we know or feel, deep down in the profundities of latent mind, or "unconscious cerebration," and only coming within the sphere of our conceptions when they have acquired a new and complex psychical nature. Without suspending this analysis, it should be here pointed out that unconscious cerebration, which appears to us to be nothing more than inattentively perceived and imperfectly remembered impression, is here made to contain or embody an instinct which has not previously existed, or which has not, at least, emerged into the sphere of consciousness. Even, according to M. Ribot, unconsciousness is not the zero of the condition, but the minimum of consciousness. If we go back from composite to simple strata in mentalisation, we arrive at what may be called rudimentary states or principles, from which all higher expansions of intelligence may be held to be derived; but there is no evidence that these two apparent extremes of the mental pole differ from each other in degree or dignity, that there has been ascent or descent from the one to the other, that there has been progression at all. These primitive states or instincts have the same relations as cell to organization, and what may have been originally reflex actions grow into habits fixed by heredity. How reflex actions grow into habits founded on volition is difficult of elucidation. It is thus affirmed, and truly, that the melipona would have gained by making its cells closer, and it is then rather summarily concluded that their gain has been accomplished in the perfect constructions of the hive-bee, by evolution. To those who are of opinion that instincts, whether simple or complex, perfect or imperfect, as well as all higher mental functions, have been from the origin of the species and formed a part of the specific type as much as brain, teeth, antennæ, &c., and that such instincts, though affected by environments, have never changed in their intimate characteristics, neither by one species improving upon the errors of another, or by new creations, these speculations are futile or unsatisfactory; but it is asserted that, plus the original mental constitution, the experience of countless generations is conserved in each individual. This is extended to individual nerves which are, hypothetically we hope, endowed with memory of the impressions transmitted along their course, and necessarily with perception and consciousness, and upon this basis is founded the proposition that the constant repetition of acts and movements, or sensation as represented as involving the latter, become so inextricably associated as to constitute habit, which is identified with heredity; nay, it is positively asserted that "the permanent effects of a par-

ticular virus, such as that of variola or syphilis, in the constitution, show that the organic element *remembers*, for the remainder of its life, certain modifications it has received" (Maudsley). In truth, the supposition that a nerve force can be transmuted into a state of consciousness is purely gratuitous, for even Tyndall admits that the chasm between love and the right-handed spiral motion in the brain is impassable. It is, moreover, fanciful to speculate upon the conservation and convertibility of moral forces as laws resembling those of heat, electricity, &c. (p. 52). A gleam of light was conceived to be thrown upon the ascension of new thoughts and combinations into consciousness, the birth of genius in fact, by what Bayne has designated spontaneous activity, having an essential connection with voluntary acts, which, if established, might be correctly called a personal factor, as originating what has not been transmitted. But, although not inclined to dispute either innateness or spontaneity, this inherent independent activity is no discovery, but is the same with, or a synonyme for, the motives, desires, pain, which excite the volition, and which whether full-grown instincts or not, arise and control mental and corporal acts, without recognisable causation; but the latitude given to modern theories upon these points may be learned from the argument that, although confessing inability in following out the transition from the period of no thought to thought, we are entitled, according to M. Ribot, to say that the organ of thought, the brain, may be modified not only by things, but by the relations of thing, space, time and causality, the relations of hought (pp. 316 & 318). Here, then, there is thought and its relations, which are the products of the organ of thought, modifying this organ, and consequently its function, through its structural and formal constituents.

It is superfluous to indicate that all such considerations invalidate the fixidity or perpetuation of heredity, and that Darwin, in affirming all acquired instincts to be insoluble or irremovable, must be incorrect, and his error is still better exposed by the history and phenomena of neuter insects, where instincts are displayed, which were not exercised by the progenitor, and which are arrested in the possessor. But even a better illustration of the fallacy and untenability of such views may be found in the progressive stages of lepidopterous insects. In the egg must rest not merely multifarous but almost contradictory laws of heredity, which pass on to the crawling, gluttonous, self-entombing caterpillar, which again pass on to the dormant, foodless, and scarcely mobile pupa, suspended by gossamer from a twig, differing altogether in structure from the matrix, but where growth and vivification gradually go on; and which are ultimately expanded in new forms, new habits and a new destiny in

the butterfly. There is even a more profound truth shrouded in these transmutations, that of identity, for that a faint shadowing of egoism must run through all the stages, may be inferred from the care of the perfect insect for its eggs, from the care of the caterpillar for the safe and suitable disposal of its successor. M. Ribot says, "that psychology, even experimental psychology, must admit a certain element which comes before us as a fact; this we call the ego, the person, the character: no other word will designate it properly, but of it we can only say that it is that which in us is inmost, and which distinguishes and differentiates us from what is not ourselves; this it is by which our ideas, our sentiments, our sensations, our volitions, are given to us as ours, and not as the phenomena of something outside ourselves" (p. 343). This seems to concede the whole matter in debate. It is quite obvious that whatever may be thought of M. Cousin's extension of the fact, the feeling of personality precedes all other mental conditions, moreover it includes all others, the entirety of conscious states; it is unassailable by growth, disease, age, decay; it may overrule and extinguish all habits and tendencies from whatever source derived, and stands prominently forward in contradistinction both to qualities supposed to be transmitted, and to qualities imparted by education and external impressions. Unless it be held that the whole soul potentially or actually is inherited, it is impossible to assign a distinct sphere to heredity in conjunction with personality. But if personal identity or egoism cannot be handed down, how can the mental conditions which constitute self? M. Ribot appears awakened to this difficulty and inquires, Is there not a *nescio quid*, the *ego* which does not and cannot pass from parent to child? But after contending that character, determinism, in fact individualism, may in some sense be transmitted, he gives up the argument as inextricable (p. 344). But there are other points at which M. Ribot makes similar concessions, such as when attempting to demonstrate the conversion of external impression into cognitions in the grey matter; where he comments upon the failure of Galton in establishing psychological heredity; where he confesses the irreconcilability of heredity with free-will as an insolvable enigma. Moreover, there frequently occur apparent contradictions and obscurities which may be referred either to difficulties in the process of translation or to the intractableness of the subject itself. Of the former, we may cite the discrepancies between the whole theory of heredity and of the declaration that people arise by civilisation to consciousness, and that it was perhaps in the last century that this was reached; although the highest act of this subjectivity, what Goethe calls converse with God, must have been coeval with

the very advent of reason. And further this defect is shown in the aid which he seeks from historians in support of heredity in races and the impossibility of changing national character, who depict the French of to-day as the same as the Gauls in the time of Cæsar, with the same levity, war-like propensity and incurable variety, forgetting altogether the individualism of Napoleon, Descartes, Laplace, &c., and his subsequent admission (p. 324) that, after eliminating all the elements peculiar to the wars, social and religious disturbances of mediæval, even recent ages, the state of the French intellect was not then the same as to-day, and that the difference between the two epochs is constitutional, organic. The degeneracy of whole nations once great and powerful, such as the Italians and Spaniards, and the extinction and the present degradation of many smaller communities, present pertinent and palpable exemptions to the assumed reign and rule of heredity and evolution, but we prefer to examine such influence and laws as traced through individuals and families. Of the latter the following may suffice. It should be premised that M. Ribot has dipped his pen so constantly in the ink of Herbert Spencer, whom he repeatedly quotes, and has become so deeply imbued with both his philosophy and phraseology, that it is difficult to distinguish between the observations of the master and his disciple. The two heredities being thus reduced to one, we again sought for the cause of heredity, and found only a hypothesis, probable indeed, but which, lying beyond the limits of experience, cannot be verified. The definite result of these researches, and the point is so important that it must be again and again repeated, is that heredity is identity as far as possible; it is one being in many. "The cause of heredity," says Hackel, "is the partial identity of the materials which constitute the organism of the parent and child, and the division of this substance at the time of reproduction." "Heredity, in fact, is to be considered only as a kind of growth like the spontaneous division of a unicellular plant of the simplest organization." Again, having introduced the words of Herbert Spencer, "And if it be asked whence these physical forces which through the intermedium of the vital forces produce the social forces? we reply, as we have all along, from solar radiation." M. Ribot says, "In a world where all things are so firmly linked together, what place is there for free-will? What right have you, say the determinists, to break up the series of effects and causes for the purpose of bringing in an unintelligible spontaneity? You say when I wish to move my arm I move it; but this movement is not as you suppose a creation—it must have already existed in your organism under a different form; and the very act whereby

you form your resolution is conditioned, is subject to determinism. There is ground for believing that every mental state is determined by organic conditions, and that consequently it comes indirectly under the laws of universal determinism. Even though you dispute this, you are in no better case, for at least you must concede that this state depends on those which precede it, and that it is subject to the laws of association, called into existence by association; but these laws of association are only one form of determinism" (p. 337).

When attention is given to the rise and gifts of heroic and representative men, to the bursting forth of genius, whether in virtue of or in opposition to surrounding circumstances, we cannot fail to be struck with the slender connection which subsists between those powers which perhaps regenerated or revolutionised the world and a superior mother or a clever grandfather. But even in the instances adduced in proof of heredity it is instructive to note that only two generations are recorded, and that where the members of the series are more numerous, flaws may be detected either in the line of descent or by the intervention of remote or extraneous influences. Where the basis is narrow the durability and tenacity of transmitted tendencies cannot be tested in contrast to the effects of struggles for moral life and of collateral interruptions. On this point Lucas says ascending movement of exalted faculties is arrested in fourth generation, and Galton has found that eminent are less numerous by one-half than illustrious men, and that such limitation may be explained by distinguished statesmen, judges, and so on marrying for money, while the daughters of highly intelligent and cultivated families are disinclined to marry at all. From such and greater and graver obstacles the stream of heredity has been so arrested, curbed, and carried from its direct course, that even in aristocracies and close corporations, which would naturally be adduced as conservative, there is no nobility said to be of earlier creation than three centuries. But, while the same names and titles may not appear in genealogies and heralds' Red-Books, many families may be traced back by indirect descent for a thousand years; although their memorials and traditions might be fairly introduced to disclose how varied the talents and tendencies of the individuals composing this long lineage have been, how inefficient noble birth and training and intermarriage are to prevent differentiation, and how often the highest attributes dwindle and die out in imbecility and decrepitude.

Proceeding upon the assumption that heredity augments intelligence by accumulation, that acquirement, or the facility of making acquirement, goes on progressively, and thus the mind

is rendered capable of further development, it has been postulated that, given one illustrious man, we may calculate the number of eminent descendants by whom he may be succeeded. It would be profitless to enter upon Mr. Galton's per-centages, or even in detail upon M. Ribot's commentaries, but certain of the facts may be examined, the real dimensions of the foundations upon which the superstructure of heredity has been built. Taking high reputation as a pretty accurate test of high ability, the three hundred families of statesmen, judges, divines, &c., cited by Galton, comprised one thousand individuals, of whom four hundred and fifteen were distinguished by talents of some kind and degree. Does this say much more than that of three hundred families, highly placed, highly educated, restrained in the choice of wives solely by rank and riches, moving in highly cultivated and polished society, in fact in the circumstances most favorable for the development of different mental powers, four hundred and fifty were gifted individuals, but gifted in various modes. In order that this should swell the proof of heredity, it would be necessary to show that the line of descent was direct, whereas cousins, half-nephews, uncles, step-brothers, and other degrees of affinity are all included in the calculation; secondly, to prove that the talent transmitted was the same, whereas judges are traced to statesmen, statesmen to litterateurs, &c.; thirdly, it would be necessary to show not merely the number, but the nature of the exceptional cases, not merely those of mediocre or non-eminent talent, but where imbecility or depravity indicated that the course of heredity had been arrested or modified. But if the different classes of which the republic of learning and genius and letters is composed be examined, still greater inconsistencies are encountered. Thus out of fifty poets, including names little known to modern ears, such as Sappho, Lucan, &c., twenty had eminent relations. But when we find that the degree of consanguinity in Ariosto was brother and nephew; in Aristophanes three sons, one of which was doubtful; that in Byron the distinction consisted in having an eccentric mother, a dissolute father, an uncle a navigator, and that all his other ancestors were either bad or mad; and that in Burns and Schiller the only imaginative influence mentioned is the sensibility of their mothers, we are struck with surprise rather than with conviction. Of the remainder it seems that six had one, five had two, one had three, three had four members in their family worthy of note, but in this estimate all degrees of relationship and kinds of noteworthiness are included. Of painters twenty-one out of forty-two had celebrated kindred. The following examples are the most important:—Bassano had four; Veronese four, but one of these failed; Caraccia five, but style totally

distinct; Murillo three, all painters; Titian nine, but this enumeration is, so far, only conjectural. Of musicians the most conspicuous examples are, Amati, with three musical relatives; Benda, with nine; Mendelssohn, with one, but these were the offspring of philosophers and authors; Mozart four, but one a failure; Sebastian Bach, possessing musical representatives for 200 years, of whom twenty-nine obtained distinction; and it is recorded that, upon one occasion, more than one hundred persons bearing his name assembled together. Allowing ample force to these anecdotes, it is probable that they embody little more than an instance of an hereditary trade.

Among men of science there occur actual exceptions to the law of heredity in Blumenbach, Berkeley, Brewster, Comte, Hegel, Hume, Kant, Locke, Malebranche, Priestley, &c. But from data gathered from biographical dictionaries and equally exceptional authorities, we learn that Arago had four remarkable relatives; but these comprise philosophers, artists, &c.; that of the nine connections of Bernouilli some were naturalists and teachers in different branches; that of the Boyle family seventeen were *savants;* that of three Cuviers one was the mother who superintended the education of the others; of D'Alembert's family, that he was the bastard of a witty woman and an artillery officer, and that one of two remaining celebrities was a dramatic author; of four Darwins one was a poet, two physicians, and one their present representative; of three Galileos the father was a musician, one the famous astronomer, and one a clock constructor; of the St. Hilaires, the father was an officer, the two sons naturalists; of the group of fifteen Gregories some were physicists, some physicians, some mathematicians; three Hallers are ranked as one lawyer, one physiologist, one litterateur; seven Herschels are divided into two musicians, five astronomers; the brothers Hunter are examples of connateness; six Jussieus may be accepted as a family of botanists. It may be said that we have arrived at the bathos or the microscopic outcome of heredity, when we describe the five Watts as including a superior mother, an astute Glasgow baillie, a professor of mathematics, *the* Watt an engineer, and his son a geologist. It should be kept prominently in view that of the causes which broke in upon the preceding mediocrity and monotomy of such lives as the Bernouillis and Boyles, and called into life and fame their great progenitors, we know nothing whatever, and that the general result is the discovery of circles rather than of successions of clever people.

The following examples may be given of the development of genius in tribes of authors. Addison's father was a learned divine, but his daughter was an imbecile, and cannot be claimed

as an example of heredity. In the two Bossuets the trace again becomes microscopic, as the one has no other merit than that of editing the works of the other. The three Brontës were of connate genius, the father being either mad or eccentric. There were four Etiennes, philologists. The Sept Grotius contained the expositor of international law; his grandfather, a scholar; his father, curator of University of Leyden; an uncle, professor of philosophy and jurisprudence; and his son, a diplomatist. Of the Hallams, the parents are spoken of as remarkable; the historian was still more so, and his two sons, one of whom was the subject of "In Memoriam," were youths of promise. Of the well-known Lambs one was a drunkard, one a maniac. Of the five Macaulays, the progenitor is said to have been an eloquent preacher in a small highland town; his son, a merchant and abolitionist of slavery, who was the father of the celebrated historian, who had an uncle a general officer. The two Scaligers indicate how short the influence of heredity, even when the quality transmitted is in its most intense degree. M. de Sevigné had three sons and a cousin notorious as clever débauchées. The De Staëls comprise one lawyer, one statesman, one authoress and wit, one mathematician, and two naturalists, one of the latter presenting a tincture of mental obliquity.

The effort to establish the heredity of propensities and sentiments may be called almost romantic. It proceeds upon a sort of gradus of petitions. If heredity be specific condition of development of intelligence, and if evolution of sentiments be in accordance with other intelligence, these sentiments depend upon heredity; and pride, and piety, and brutism may run in the blood parallel to scrofulus taint. What is designated the acquired feeling of fear, the proof of such acquisition depending upon the pseudo-fact that the Falkland Island dogs had no fear—not, in fact, until they were frightened—has been affirmed in utter forgetfulness that there are tribes of animals naturally timid as well as others naturally courageous, and that not merely in the same species, but in the same litter different individuals display totally different degrees of courage. Whimsical peculiarities in men and animals, idiosyncrasies, antipathies, are all resorted to in order to illustrate the principle, but, so indeterminate are the states of our moral nature, that it is impossible to prove transmission. It is true that dipsomania, or the involuntary tendency to inebriation, has been seen in several members of the same family, but the propagation of this morbid craving can most frequently be seen in certain of its consequences, such as insanity, idiocy, paralysis, and physical defects. It is likewise well established that whole clans, even sections of the community associated by blood and breeding, are sunk in crime,

but when the intricacies of this tangled and rotten web are unravelled it becomes apparent that the kind of crime may differ widely. In fifteen offenders, named Chretien, the depravity appeared in four as murderers, with or without robbery; the wife of one of them was of an incendiary stock; in six as robbery or theft; and one died a violent death. To these may be added that kleptomania has occasionally been seen as the offshoot of great genius or insanity, and has been associated with brilliant parts.

That a similar effort to demonstrate the heredity of will is open to the same objections and exceptions may be learned from an epitome of the ancestors and collaterals of statesmen and generals, of whom this quality is regarded as the prominent characteristic. Colbert, the distinguished minister, had a brother, a statesman; a son, a general officer; another, an archbishop; and a nephew, a diplomatist. Of Cromwell the descendants were of mediocre capacity, but Waller, the poet, was the nephew of his cousin, Hampden, the patriot. The Lamoignons, numbering twenty or more, all embraced the exercise of law, justice, or charity as professions, and there was, besides, one precocious child. It may be fairly suspected that their biography would disclose position, nepotism, and cliquism, and family interests and arrangements, as adjuncts to any general law. The same remark is applicable, with some modifications, to the Medicis and Peels. The five Sheridans are represented by the grandfather, who was a friend and correspondent of Swift's; the father, who wrote a dictionary and was manager of Drury Lane; the brilliant orator himself, his son Tom, defined as a Sheridan all over, and the Honorable Mrs. Norton. The following commentary by Galton will expiscate the bearing of this genealogy upon the topic we are considering. "The name of Sheridan is peculiarly associated with a clearly marked order of brilliant and engaging, but 'ne'er-do-weel' qualities, reckless prodigality, gambling, and wild living. . . . These qualities are found in a greater or less degree among numerous members of the Sheridan family." (P. 120.) Berwick, as an illegitimate son of James II, and of a daughter of the Duke of Marlborough, may claim a royal as well as a military origin. The Bonapartes had an astute mother, but deserve consideration rather from their connate birth than hereditary talent. Of the Colignys, grand and pure though the admiral may have been, it is enough to say that they were a family of princes. Prince Eugène, classed by Napoleon with Turenne and Frederick the Great, had the priest-politician, Cardinal Mazarin, as a grand-uncle. The kingly race who bore the name Gustavus comprehended patriots, heroes, philosophers. The reputation of

Napier, the discoverer of logarithms, was handed down through eight generals and statesmen. Besides his relationship to the house of Orange, Turenne derived lustre from his father the leader of the Huguenots. But how immeasurably separated in kind are the wisdom and the bravery or warlike abilities of these statesmen and soldiers.

Even the details of deaf-mutism so triumphantly referred to in such investigations are more specious than convincing, for of 148 pupils in the London Institution at one time, there was one in whose family were five deaf mutes; another in whose family were four; in the families of eleven of the pupils were three each, and in those of nineteen two each. Were the comparatively insignificant number of the instances of Daltonism, sex-digitism, and of porcupine skin subjected to a similarly crucial examination, our experience would be very nearly the same.

It is marvellous that when the elaboration of the descent of mental tendencies and faculties has been worked out with so much solicitude and ingenuity that so little importance has been attached to education, training, example, imitation, even physical impressions, position, stern poverty, in creating and constituting character, in correcting passions, propensities, in engrafting tastes and knowledge, in short, in transforming whatever may be due to Innéité, heredity, or spontaneity, into new powers, in giving infinite variety and multiplicity to human development, in making man to differ as much from the primitive mode or type and from all the links by which he is connected with it, as the mature differs from the infantile and the civilized from the savage mind. It would be rash with Lamarck and his followers to refer all physical and moral changes to the environments, to the exclusion of native energy; or to assert, with Locke, that of a hundred men ninety are good or bad in consequence of external circumstances, but it is certain that, without the co-operation of these, progress or evolution in its non-technical sense would be a mockery. Even one of the grand arguments advanced in favour of heredity, the results of consanguine marriages, appears to militate materially against the theory. The application of this argument formerly assumed the form of inferring degeneracy as their inevitable result. This conclusion is now held to be erroneous, but had it been true it would have merely proved the danger or evil of the union of two diseased constitutions. But, in the first place, consanguinity in breeding animals is found to be advantageous in the development of new qualities, as size, beauty of form, increased secretion, obesity in sheep and oxen; and gracileness and speed in Race-horses where intercourse has been going on for two hundred years. And, in the second place, putting out

of view the castes in India where separation is maintained more by custom, occupation, habits and cultivation, than by race, it is well ascertained that while intermarriage even between near relatives has gone on for centuries among the Jews, small Moslem communities in Hindostan, the inhabitants of the coasts of Barbary, &c., there exists as much dissimilarity in form and features, and as much individualism in faculties, motives, and course of action, as can be detected in other races and nations where such unions are not tolerated and occur very rarely.

By a certain class of thinkers M. Ribot's work will be hailed as the most profound disquisition upon the many subjects embraced which has ever appeared; by all it may be accepted as an attempt to produce a philosophical disquisition on what has been vaguely and fragmentally believed. There is a constant and strained effort to prove and explain much that must remain for more advanced stages of science, and the result has been that rash or premature hypotheses have been ventured upon where data are insufficient or obscure, and many of the controversies now agitating the world are treated as if adjusted, and what are still mere postulates are built upon as propositions either demonstrated or demonstrable. In dealing with the author's speculations, which we regret are more metaphysical than logical, it has been our aim, while embodying his particular opinions, avoiding what appeared paradoxical in them, to point out the various grounds which render it impossible to admit heredity otherwise than conditionally and provisionally, without adopting a positively hostile or antagonistic course. It may be readily conceded that heredity exists, so far as the characteristics of species, genera, and classes are concerned; but we must pause when called upon to believe that "as what intelligence, fixed by memory, is to the individual, progressive experience, fixed by heredity, is to the species." We would willingly receive a sketch of the ascertained phenomena and their relations in heredity similar to the tracing made by navigators in the Arctic circle of lands certainly discovered, but separated by gaps when the shore line has been imperfectly observed, or has not been seen at all; but we must decline a chart filled up and finished, but which unites imaginary continents and perhaps dream-land with the solid basis on which the truths of physiology and philosophy at present rest.

Notwithstanding these objections, the chart actually before us is ably executed, is in many of its features interesting and attractive, and should a second edition be required, as may be expected, and be pruned of certain obscurities and transcendentalisms in theory and expression, the work would be fairly entitled to rank beside or above the valuable treatise of M. Lucas.

VII.—Review of Sanitary Work in India.[1]

The registration of the deaths of the civil population in those provinces of Hindostan, under British rule, which was commenced in 1865, has since been steadily continued. It was at first very imperfect, and though considerable improvements have been effected, the results are still very far behind what we are accustomed to in this country; nevertheless, the compilers of the various reports which form the basis of these remarks all agree that it has afforded a view of the varying distribution and incidence of certain diseases, far exceeding any previously available, the importance and value of which is becoming apparent in the modification of opinion which it is gradually leading to. We shall endeavour, in the remarks which follow, to place a few of these before our readers, and to point out their bearings on questions agitating medical opinion throughout the world.

As might be expected, cholera forms the chief topic in the Indian reports, and while that country presents an unrivalled amount of material for its study, the system of registration now in operation over an area nearly as large as Europe, though interfered with at various points by districts under native rule, still affords a continuous and uniform record of its visitations which has not yet been attained elsewhere. Though there can be no question that, for the complete investigation of cholera, the course of epidemics in every other country as well as in India must be studied, yet it is of vast importance to have secured a fair record of its manifestations in so extensive a field, with which the scantier notices from neighbouring regions may be connected in the first instance, and ultimately brought into their proper places in the general system. In the Ninth Annual Report of the Sanitary Commissioner with the Government of India, the author, Dr. Cuningham, writes:

"It is only, as I have before remarked, by collecting every fact

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[1] 1. *Ninth Annual Report of the Sanitary Commissioner with the Government of India,* 1872. Calcutta, 1873.

2. *Tenth Annual Report of the Sanitary Commissioner with the Government of India,* 1873. Calcutta, 1874.

3. *Report of the Sanitary Commissioner for Madras,* 1872. Madras, 1873.

4. *Report of the Sanitary Commissioner for Madras,* 1873. Madras, 1874.

5. *Ninth Annual Report of the Sanitary Commissioner for the Government of Bombay,* 1872. Bombay, 1873.

6. *Tenth Annual Report of the Sanitary Commissioner for the Government of Bombay,* 1873. Bombay, 1874.

7. *Annual Report of the Sanitary Commissioner for the Central Provinces,* 1873. Nagpur, 1874.

8. *Report of the Sanitary Administration of the Punjaub for the Year* 1873, Lahore, 1874.

that can be gathered regarding an epidemic from its commencement to its close, that we can be in a position even to attempt to understand the phenomena which it presents. The collection of facts from all countries attacked, facts observed and narrated apart from and unbiassed by any theory, is of the greatest importance. If we only knew *all* the facts, the framing of correct conclusions would be easy." (Par. 114.)

Then with reference to cholera, he says:

"Its sudden and fatal character has made it appear as if cholera were more mysterious than any other disease, but it is just as difficult to account for the epidemic spread of other diseases, of epidemic malarial fever for example, or for smallpox, as it is for cholera." (Ib., par. 116.)

And further·

"Smallpox, it is argued, is a contagious disease, and spreads only by contagion. Cholera belongs to the same class of contagious diseases, therefore it also spreads by contagion, and by contagion only. But do we know that smallpox spreads only by contagion? Its annual rise and fall in this country recurring steadily year after year, its comparative dormancy for a term of years, and then a year of epidemic violence, are facts altogether inexplicable on the doctrine of contagion, and prove incontestably that the law of contagion is not that law which governs the spread of smallpox. The obscure problems connected with the epidemic prevalence of disease are to be solved, not by fancied analogies, but, like every other truly scientific inquiry, on the evidence." (Ib., par. 117.)

In the Ninth Annual Report for India, the course of the cholera epidemic of 1872 is detailed, and in the Tenth the distribution of the disease in 1873; we cannot extract these here, but may render their more striking features apparent by giving the number of deaths registered each year in the various provinces into which the country is divided. In the following table these are arranged from north to south, as nearly as can be, except British Burmah, which being to the east of the Bay of Bengal, cannot be placed among the others; the deaths registered in 1871 have been added to give a more extended view of the course of the disease.

	1871.	1872.	1873.
Punjaub	369	8,727	148
North-western Provinces	3,473	50,565	15,268
Oudh	16,032	26,566	3,961
Bengal Proper	20,396	46,901	64,366
Central Provinces	19	1,592	344
Berar	581	1,578	—
Bombay Presidency	5,855	15,642	283
Madras Presidency	17,656	13,247	840
British Burmah	162	640	8,109

The distribution of cholera in these years, as shown in the

maps accompanying the Reports of the Sanitary Commissioner with the Government of India, is very striking and instructive. In 1871 there was a continuous body of the disease extending from the southernmost point of the peninsula up to the 19th parallel of latitude, from the east coast to the meridian of 80° E., and west of that, to a line passing north-west from the point where the 19th parallel intersects the 80° meridian to the river Taptee, and along that to the sea. In this extensive area the whole of the table-land of the Mysore and the west coast districts, extending from $11\frac{1}{2}$° to to $16\frac{1}{2}$° N., and from long. 78° westward to the sea, was free from the disease. Another cholera field occupied Bengal proper, Oudh, and part of the North-western Provinces; this was bounded by a line extending from the coast at Pooree north-westerly to the meridian of 80° E., in lat. 27°, and that parallel indicates the northern limit of the epidemic to the eastward. Thus not only was the Mysore free from the epidemic, though surrounded by it on three sides, but right through Central India the two cholera fields were separated by a tract of some 400 miles in width, in which at most but a few sporadic cases were met with.

In 1872 all this was changed; cholera as an epidemic had disappeared from the southern part of the peninsula, unless in a mild form, and in a limited district between the rivers Cauvery and Palar, and occupying half the space from the scarp of the Mysore table-land towards the coast. After a considerable interval it commenced again in the low land on the east coast, north of the Kistna, and occupied this tract as far as Pooree, where it joined the epidemic area of Bryden; to the westward it had occupied a portion of the space left clear in 1871, and its eastern margin came up to a line, from lat. 13° on the west coast, extending north-east to the point where the 20th parallel of latitude cuts the meridian of 80°, then due north to where that meridian meets the 24th parallel, then north-westerly to lat. $25\frac{1}{2}$° on the meridian of 75° E., from which it curved down to the sea at Surat. The Bengal cholera field of 1872 had retired from the high ground of Chota Nagpoor, which lies between the coast and the valley of the Ganges, but had also extended to the north-west as far as the Indus, just touching Mooltan on the one hand, and Peshawur on the other, but nowhere approaching that previously mentioned within 100 miles. In fact, including the limited outbreak in the south, there were three continuous cholera fields in Hindostan this year, two of them of very great extent, and which manifested considerable invasive powers, yet at no time did they coalesce at a single point.

In 1873 there is again a marked change on the face of the

map, the whole of Southern India being free from epidemic cholera, except a slight outbreak in the Nellore district in January and February, causing 476 deaths, and a much slighter one in the Kulladghee district in the south of the Bombay presidency, where 90 deaths occurred from January to March, and Bombay (city) itself, where 93 deaths were registered in the course of the year. In the north there was a severe epidemic in Bengal, extending into the North-western Provinces; its western limit was a line extending from Ganjam on the coast to Agra, and from that point north-east to the high ground, along the base of which it skirted more or less closely to the south-east. Coincident with this epidemic was the severe outbreak in British Burmah.

With his previous acquaintance with cholera, and having the details of the 1872 epidemic, of which an outline has just been given, before him, Dr. Cuningham, the Sanitary Commissioner with the Government of India, was induced to express his doubts as to the soundness of the opinion then entertained by various authors as to the causes of cholera, and, in his Report for 1873, after the further experience of that year, he gives his conclusions in the following terms:

"1st—That if human intercourse plays any part in the dissemination of cholera, it must be a very secondary part.

"2nd—That the facts of individual outbreaks, and especially the remarkable immunity of the attendants, are altogether opposed to the doctrine that the disease is spread by communication with the sick.

"3rd—That the theory which would explain the phenomena on the supposition that in a year of epidemic prevalence very many sources of water-supply over an enormous area had been polluted with cholera discharges, while in a year when cholera is dormant such pollution has either been rare or has not taken place at all, fails altogether to explain the facts either of wide-spread prevalence or of individual outbreaks.

"4th—That there is no evidence to show that a person affected with cholera multiplies within himself any specific poison, or that he disseminates any such poison either by means of the intestinal or any other discharges.

"5th—That cholera in India appears to be due to certain conditions of air and soil, or of both combined, as yet little understood, which are always more or less present in the Lower Provinces of Bengal—the endemic area, and which appear only occasionally in the Upper Provinces, the intervals between the times of their appearance increasing as we proceed to the North-West, and being so short in the immediate neighbourhood of the Lower Provinces that it is impossible to define exactly where the endemic limit ends.

"6th—That these conditions would appear to be often singularly localized both in the endemic and epidemic areas, as is evidenced by the very local character which so distinguishes outbreaks of the disease.

"7th—That the great danger arises from exposure to these conditions, and not from exposure to any emanations from the sick.

"8th—That even if the contagious character of cholera could be proved beyond all manner of doubt, any general system of quarantine sufficiently strict to be effectual is impracticable, and must do much more harm than good.

"9th—That the great safeguards against cholera are sanitary improvements—the improvement of drainage, of water supply, of dwellings, of everything, in short, which can contribute to health. Of water supply it may be remarked, not that it may be safe only against cholera evacuations and the results which have been theoretically ascribed to them, but that it may be safe against every form of impurity." (Pages 17, 18.)

In his Report for 1872 Dr. Cuningham anticipated the announcement of these views would call forth vigorous protests from those who entertained the opinions he questioned, and that they would complain that if we accept his position we virtually go back to the state of ignorance we were in many years ago. To this he replied that "if we are on the wrong road, the sooner we go back the better, but the question is not what will be the consequence to any opinions. The only point of importance is—what is the truth? Facts cannot be too carefully scrutinized before they are accepted, nor conclusions too carefully drawn from them." (Par. 118.)

The real cause of difference between Dr. Cuningham and his opponents lies rather in the conclusions they consider themselves justified in drawing from certain facts, than in their recognition of the facts themselves, always supposing that these have been carefully ascertained, and are not what we meet with so often in descriptions of disease, basements of fact with superstructures of fancy, which are deserving of no weight in any scientific investigation. We shall endeavour to place the views now springing up in India on this question before our readers, that they may be in a position to judge for themselves which party is pursuing the proper course to clear it up.

In the fifth conclusion, given above, Dr. Cuningham states his belief that, in India, cholera appears to be due to certain conditions of air and soil, or of both combined, as yet little understood, which are always more or less prevalent in Lower Bengal, but which appear only occasionally in the Upper Provinces. These conditions appear to exist in some other parts of India as well, for instance, in the South Arcot district in Madras, where, from the commencement of the registration of deaths, in 1866, to the end of 1873, there has not been a single month without one, but much more commonly numerous deaths from cholera; and in the Tanjore district, which adjoins it, in

the same period, August, October, November, and December, 1872, and January, 1873, were the only months in which there was no death from cholera registered; the district of Trichinopoly, in the same locality, had deaths registered every month to the end of 1872, but none during 1873, so that for eight years, at least, the causes of cholera had been permanently in operation in this locality. For the city of Bombay, too, the monthly returns of deaths from cholera are available from 1848 to 1873, inclusive (with the exception of 1866), or for twenty-five years; during that period there were seven months in 1848 and 1849 without a death, but since then only December, 1867, and February, 1868. The disease has undergone great fluctuation in both those localities in the periods mentioned, as it has done in what Dr. Bryden has denominated the endemic area in Lower Bengal.

It will occur to every inquirer to ask what leads to these fluctuations—what could have led to such an extensive development of cholera throughout Hindostan in 1872, the incidence of which in 1873 was so altered that cholera had almost disappeared from Southern, Western, and Northern India, while a severe epidemic prevailed in Bengal and Burmah? It was a point much urged by a certain school of epidemiologists that cholera never travelled faster than man, and the Constantinople Conference, in 1866, was rash enough to commit itself to the opinion that now the means of locomotion were greatly increased, and much more rapid than formerly, it would be found that the diffusion of cholera, and other epidemic diseases would be accelerated proportionately. Facts existed at that time which should have caused the delegates to hesitate before coming to this conclusion, and the experience of Europe since then has shown so completely that extensive and rapid communication is insufficient to cause the epidemic diffusion of cholera, that the late Conference at Vienna, with greater caution, fell back on the former position, and did not reassert the latter. The experience of India is altogether to the same effect. The lines of railway now open in that country enter and leave the various cholera-fields without indicating the least power to advance the limits of these fields beyond the line which marks them out for a long way on either side of the railway, and far beyond its influence on the direction of the traffic. In 1871, for instance, the railway from Calcutta to Allahabad was in the cholera-field, from Allahabad to Cawnpore it was at its outer margin, and from Cawnpore northwards the cholera-field would not extend, the railway facilities notwithstanding. Similarly, between Allahabad and Bombay, the railway left the cholera field a little south of Allahabad, then ran through a country for about 400 miles

free from the epidemic, after which it entered the other cholera-field at that time existing in the Bombay presidency. In 1872, when there was little cholera in the Madras presidency, the railway from Madras to the west coast at Beypoor passed through the cholera-field between the Palar and Cauvery rivers without conveying the disease beyond these streams towards either terminus. This year, too, though the Bombay cholera-field had advanced considerably to the northward, there was still a wide zone between that and the other field including Allahabad, through which the railway ran, but to which the epidemic did not extend. In 1873 this feature was repeated; the railway from Allahabad to Bombay, after running about 100 miles in the cholera-field, suddenly left it without causing any extension of the epidemic towards the latter place; and the line from Allahabad to the Punjaub left the cholera-field at Agra in the same manner; there was a slight isolated epidemic in the Delhi and Goorgaon districts, as had occurred elsewhere this year, but beyond these places there was none. In addition to these recent facts, Drs. Cuningham and Bryden have frequently stated that their investigations have shown that cholera does not travel faster in India now than it did fifty years ago, when locomotion was much less rapid, and that, even at the present day, in its epidemic form, it spreads over portions of country beyond the influence of railway traffic quite as fast as through those in which the railways run.

Again, though the traffic of the country goes on from year to year, with little change in those parts of it which have not been affected by railways or steam navigation, and much as it went on many years ago, that traffic seems unable to extend the limits of a cholera-field, as determined for the time by circumstances we are as yet but little acquainted with, so as to embrace neighbouring populations within the spread of the epidemic. No one can look at the map for 1873, for instance, without feeling surprised that the traffic, which must have gone on much as usual from the districts under the epidemic to those around them, failed to cause the extension of the epidemic into these, were all that is necessary for this purpose the mere introduction of persons labouring under the disease. There are many cases of individuals, who have been exposed to the causes of cholera in one locality, moving into another where these did not seem in operation, before the disease became developed, yet, when this took place, cholera did not spread among the surrounding population so as to become epidemic. Dr. Townsend, the Sanitary Commissioner in the Central Provinces, in his Report for 1873 gives the following case:—In the end of January the head-quarter wing of the 31st Madras Native Infantry, left Berham-

pore for Raepore; at Sonepore, on February 8th, cholera appeared among the men of the regiment and the followers, and up to the 19th inclusive there were 81 attacks and 31 deaths, of whom 5 were soldiers and 26 followers. During this time the wing continued its march, and, by the 19th, reached the river Jonk, when the disease ceased. After the troops had passed, a number of cases appeared among the inhabitants of the villages bordering on the route followed by them. Some of these had been in communication with the troops, others not. Dr. Townsend, who thinks importation is largely concerned in spreading cholera, naturally concludes that this regiment introduced the epidemic into the district, assisted by the pilgrims who at that time were returning from the Dhole Jatra festival, which is held at Pooree annually, in February. The inhabitants were instructed to maintain cleanliness in their villages, to prevent bathing or washing of clothes in the tanks or pools from which their drinking water was obtained, and to prevent communication with infected places or people. With reference to these recommendations Dr. Townsend remarks:

"How far the measures of prevention which were adopted by the District Officers and by the people themselves contributed towards limiting the duration of the epidemic and the area over which it spread cannot be determined, but that they were to some extent effectual seems probable from the circumstance that while in former epidemics, which commenced with the return of the pilgrims from the Dhole Jatra in February and March, the disease continued prevalent through the hot weather and rains, the epidemic of 1873 began to decline as soon as the stream of pilgrims had passed through, and it had disappeared altogether by the middle of April." (Par. 88.)

And he adds the following most important remark:

"Moreover the fact that although cholera was brought into a large number of villages it spread in comparatively few, plainly shows that the local conditions which favour the spread of cholera were not at the time common in the district." (Par. 89.)

This brings us face to face with something else than the supposed introduction of the germ or contagium of cholera by the troops and pilgrims, and without the co-operation of which, though the disease obtain a footing, it cannot maintain itself. Dr. Townsend, we believe, refers to water impure from any form of decomposing animal matter (not necessarily cholera evacuations), but, if so, with his description of the condition of the supply in the Central Provinces, this state of the water must be present to a great extent in the early months of every year in the Sumbulpur district, where the occurrences under consideration took place, and there is no alternative but to fall back on

something still more general in its operation. Under such circumstances are not those epidemiologists who trust to personal communication, or water contaminated with cholera discharges, to explain the spread of epidemics, open to the criticism that they confine themselves to minor questions, and attribute to these the influence exercised by the more general cause or causes just mentioned, and so invest them with a degree of importance they are in no wise entitled to? No one investigating a point in any other branch of natural science, who found himself confronted by a cause controlling all the supposed minor factors patent to him, would think of assigning to these the sole influence in producing the result; on the contrary, he would be aware that until he had acquired a tolerably precise knowledge of the mode of operation of the more powerful cause, he could form but a very imperfect idea of how far the minor factors contributed to the result, or even whether they were in any way essential to it. In epidemiology, unfortunately, these considerations are too often disregarded, and, as a consequence, with all the zeal and activity expended upon it of late years, our progress in generalizing has been too often slow, if not sometimes actually retrograde.

Dr. Townsend would, no doubt, take up the position that whatever may be the extent of operation, or nature of the influences he refers to, still the introduction of the contagium of cholera is necessary. We find, however, that not only he himself refers to cases which have appeared without any known communication with others labouring under the disease, but Mr. Cornish, who also considers cholera is spread by man, in the Report for Madras for 1873, tells us that "there is in South India an endemic form of cholera which seems to differ from the epidemic variety only in not spreading" (par. 394); that is, at a time when cholera is not epidemic, cases spring up sporadically, without any known connection with previous cases, which are not distinguishable from those which occur during an epidemic. Their causes, then, so far as our knowledge goes, are connected with the locality or habits of the individual attacked, and independent of the occurrence of previous cases. With such a fact before us for one part of India, and with similar evidence from many others, if not so constant yet open to as little doubt, we think those who insist on the necessity of the introduction of the germ or contagium of the disease by persons or fomites, are under this difficulty, that they have failed to exclude the local origin of the disease, and until that be done, they can never claim for communication by personal intercourse, anything beyond a low degree of probability.

It is frequently alleged that malignant cholera, when it occurs sporadically, differs in its nature from malignant cholera when

epidemic; but is there any ground for such an opinion? No one, for instance, considers a sporadic case of smallpox to differ from those met with during an epidemic, because, under the latter condition, the cases are more numerous; this fact merely indicates that there is something at work which then determines the more frequent occurrence of cases, without proving there is any difference in their nature. Hitherto all attempts to show a distinction between the cases of malignant cholera which appear sporadically and those which constitute an epidemic have failed, and therefore we are not justified in assigning to the latter a power which we admit the former do not possess, merely to accommodate them to a theory. As with smallpox, the only inference the facts authorise is that, when epidemic, there is something which determines the greater frequency of the disease than when it occurs sporadically, not being an attribute of the individual cases, but independent of them, and which may be in operation without any cause appearing, though we cannot be certain of its being so in any locality unless we see cases arise. We shall, no doubt, be met with the old objection here, that were such an influence, or series of influences in operation, every place within their range should show their effects by having cholera developed in them; to which we reply, that if we wish to ascertain the process followed by nature in the production of epidemics, whether of cholera or other diseases, we must not start by laying down the limits within which *we* think she should confine her operations, but must endeavour from a close study of phenomena to find out the limits which she herself has imposed.

The description given above of cholera in 1872 and 1873, in Bengal, leaves no doubt of its causes having been unusually active both years, particularly so in the latter. In 1872 the districts of Sambalpur and Bilaspur were not included in the cholera field, but in 1873 the boundary of this extended somewhat to the south-west, and embraced the former, while it just touched the latter. The course of the epidemic in these, and the adjacent districts in the Bengal Presidency, is shown by the following numbers from the monthly returns of deaths in 1873.

	Jan.	Feb.	March	April	May
Balasore	67	255	416	185	95
Cuttack	164	343	640	242	55
Pooree	40	84	70	14	—
Sambalpur	—	40	190	69	—
Bilaspur	—	—	20	—	—

So that there was considerable mortality from cholera at its appearance in January, which, in Balasore and Cuttack, went on increasing till March, after which it declined till May. In

Sambalpur the disease commenced in February, but culminated in March, and fell off in April; while Bilaspur was reached only when the epidemic was at its height in the other districts. The same wave, therefore, which was experienced at Balasore and Cuttack, and to a smaller extent at Pooree, extended to Sambulpur in a slighter degree, and was just felt in Bilaspur. Dr. Townsend thought the cholera was introduced into Sambalpur by the troops, assisted by the pilgrims returning from the great festival at Pooree, in February and March. Now, in February, 1872, there was almost as much cholera in the Pooree district as in February 1873, and the probability of pilgrims contracting it there would have been much the same in both years, yet in 1872 they returned through Sambalpur (not included in the cholera field that year) without any appearance of the disease in their track, while in 1873, when Sambalpur was embraced in the cholera field, both pilgrims and residents began to suffer, and Bilaspur beyond it and outside the cholera field, but through which a stream of pilgrims passed, was scarcely affected. Thus this history is merely a repetition of the experience afforded by the railways, and, like that, shows that persons who have been exposed to the causes of cholera within a cholera field may proceed beyond that before the disease becomes apparent, but that no active development of the malady takes place unless within those limits which bound the field, and which vary from year to year quite independently of the usual course or amount of the traffic. We, therefore, think Dr. Cuningham is quite justified in announcing "that if human intercourse plays any part in the dissemination of cholera, it must be a very secondary part."

Dr. Cuningham objects, in his third conclusion, to the theory which attributes the spread of cholera epidemics solely or chiefly to the employment of water contaminated by cholera discharges. That the use of such water during an epidemic period may be followed by cholera in some of those who drink it, we presume he would not deny, but that it contains the germ of the disease, which so finds a way of entrance into the system, he is not prepared to admit. The evidence adduced on this point, we confess, has always appeared to us as insufficient to establish it; water contamination can never take place without a first case, and the causes which sufficed to originate that are quite ignored when we come to talk of fresh cases supposed to have been excited by the water into which the discharges from it had passed, though these causes may still continue in operation. When cholera is prevailing, or threatening, anything which will derange the stomach or induce diarrhœa is very apt to excite it, and in India it is well known that at such times a dose of any

of the usual saline purgatives, or even of the milder vegetable laxatives, will frequently lead to cholera, and decomposing organic matter, whether animal or vegetable, taken as food or in water, frequently does the same. All we know of the efficiency of cholera discharges in exciting cholera amounts to no more than this, that when taken into the stomach they are inert when quite fresh, but after undergoing a certain amount of change they acquire the property of lighting up the disease, and, again, that after a certain period, this property is lost. But those phenomena only take place when cholera is already present, and, at such times, the use of decomposing meat or fish, or ascescent fruit, may be followed by the same result, though there be no reason to suppose that any of these have been in contact with cholera discharges.

Drs. Lewis and D. Cuningham have injected cholera discharges, both fresh and putrid, unmixed and diluted with water, into the veins of dogs, and found that in all these conditions they produce, in a certain percentage of cases, an intestinal affection which they denominate "hæmorrhagic gastro-enteritis," the lesions and attendant phenomena of which they state are not identical with those of cholera. Drs. Lewis and Cuningham also injected a solution of fæculent matter from a healthy man, both fresh and putrid, and found it produce results indistinguishable from those following the introduction of the cholera discharges, only in a reduced proportion. To ascertain whether these results were dependent on the presence of any germ, or other living organism, which might increase in the system, both the cholera and healthy discharges were, in a number of experiments, boiled immediately before injection; in these the toxic effects were even more decided than when the infecting material had been injected without having been heated previously. The general result showed that 45 per cent. of those animals in which cholera material was injected were affected, whilst only 27 per cent. of those in which healthy fæculent matter was employed suffered. With reference to these experiments these gentlemen remark:

"Why material, whether choleraic or non-choleraic, should exert its power in some instances and not in others, or why choleraic material should appear to possess this power more frequently than ordinary material, we cannot explain. . . . Something, however, is present which, as we have already said, is capable of exercising a singularly pernicious effect on animal life, the most prominent local manifestation of its action being observed in the intestinal canal." (*Tenth Annual Report of Sanitary Commissioner with the Government of India,* Appendix A, p. 107.)

And again—

"Therefore until it be found that living substances can withstand immersion in a fluid at a temperature of 212° F. of some minutes' duration, we have no hesitation in stating that the morbid phenomena which we have observed to follow the introduction into the animal economy of strained solutions of choleraic and normal alvine discharges, and of other decomposing animal substances, are not the result of infection with a material the poisonous properties of which are dependent on its possessing vitality. (Ibid., p. 108.)

Drs. Lewis and Cuningham have made many examinations of the blood, both from healthy persons and from cholera cases. They found the chief character of the latter to be an unusual quantity of white corpuscles, and what they describe as a "diffluent condition of the red corpuscles" which "showed itself in a tendency manifested by the corpuscles to aggregate in irregular masses in place of forming the normal rouleaux, and in ordinary preparations where any pressure was exerted, and in which there was any movement of the fluid, in the ease with which the corpuscles altered their forms, were drawn out into irregular processes or adhered to one another by elastic protrusions" (Ibid., p. 82). There was a complete absence of bacteria, or fungi, or other extraneous bodies.

We imagine few can examine the maps showing the varying distribution of cholera in India from year to year, attentively, without being led to the inference that there must be some one or more factors of extensive operation concerned in producing such general results. As yet Dr. Bryden is the only person who has endeavoured to indicate the nature of this general cause, so far as cholera is concerned, and, as many may be aware, he believes that a material is produced in the endemic area in Lower Bengal which the natural currents of the atmosphere carry through the country, and that when deposited in a suitable locality, under favorable circumstances, it is reproduced, causing cholera among those who are exposed to the new product, and also affording seed for a fresh growth at the spot, or for being carried to a farther point, where, under suitable conditions, the same process would be repeated. Dr. Bryden thinks that this material, conveyed from the endemic area by the winds at various seasons, may account for the distribution of cholera, not only over Hindostan, but also over other countries, including the East Coast of Africa, and Islands in the Indian ocean. What leads to the issue of this body of cholera (as Dr. Bryden occasionally calls it) from the endemic area being repeated at intervals of several years, Dr Bryden has not mentioned, though it is clearly a very important question. Dr. Cunningham evidently thinks well of Dr. Bryden's theory, though he does not consider it has as yet been fully established. Its chiefest opponents have

been those who believe they could explain the varying phases of cholera epidemics by personal communication, or by water supply contaminated by cholera discharges; as these gentlemen, however, are now beginning to admit the necessity of something else to account for the spread of cholera, it is not improbable that ere long the operation of general causes may be commonly admitted, and the course of investigation placed on a more correct basis than hitherto, when the facts we have to deal with will be interpreted in quite a different manner from what has been customary of late. We believe this change is impending, and that its advent will be hastened in no inconsiderable degree by the results of the registration of deaths in India, and by the outspoken manner in which Dr. Cunningham has expressed the opinions which the study of those returns has enabled him to form.

The year 1872 was remarkable for the very extensive prevalence of Dengue in India, and elsewhere in the East. In 1824-25 it had spread extensively through the East, and, subsequently, it seems to have appeared at Calcutta in 1836, 1844, 1853-4, and isolated cases were met with almost every year. On this occasion the first report of Dengue having appeared as an epidemic was received from Zanzibar, where it broke out, abruptly, in July, 1870, after the termination of the cholera epidemic of that year. The next account of it was received from Aden, where it commenced in the middle of June, 1871, and, after prevailing very extensively, ceased in the middle of September. In November it was frequent at Port Said on the Suez Canal. At Calcutta one case at least was met with in September, 1871. It was only in 1872, however, that Dengue became extensively diffused in India; it was first remarked in a detachment of European troops sent from Bombay to Cannanore, in January, in the Dalhousie steamer. On landing the cases were segregated, and there were but two or three subsequently among the residents in the cantonment; there were thirty cases in January, and one in February, and no others there during the year, either among the European or native troops. There was a single case at Rangoon in February among the European troops, but it was only in April they became frequent. At Calcutta there were a few cases in March, but in April they became more numerous; the North-western Provinces were not affected to any extent until July and August; the epidemic culminated in them in September, after which it gradually declined. The furthest point reached by the disease was Loodianah; at Umballa there were a few cases in October, November, and December, among the European troops. Among the native troops, and prisoners in the jails, the distribution and period of occurrence of the

disease seem much the same as among the European troops. In the Bombay Presidency Dengue first appeared at Poona, in April, during which about a tenth of the European troops were attacked; in Bombay itself they became affected in May; and in the Madras Presidency the epidemic (except the outbreak at Cannanore already mentioned) began in April. The Straits Settlements were affected early in the year, and the disease was common in China, at Shanghai in June, at Amoy in August, and at Mecca among the pilgrims about February. In 1873 there was a slight return of the malady, but its force was much reduced everywhere, and the number of stations affected was much smaller in the Bengal and Bombay Presidencies, but the epidemic still retained considerable, though diminished force, in that of Madras. The large expanse of country from the Punjaub down to the Central Provinces was scarcely at any time under the influence of the disease.

Mr. Cornish, in the Report for Madras for 1872, points out the extraordinary immunity from Dengue of the prisoners in the jails. Those in the Madras, Nellore, and Tanjore jails only were affected. At Vellore, Dengue prevailed among the civil population from May to October, the jailor and many of the warders were attacked, but none of the prisoners, the average strength of whom was 943. At Trichinopoly, three fourths of the people in the town were prostrated by the disorder; the native troops also were very extensively affected, but not a single case occurred in the jail, where the average number of prisoners during the year was 875; and at many other stations where the native troops suffered to a large extent the prisoners escaped altogether. When European and native troops were quartered at the same station the latter were attacked in larger proportion than the former. The comparative immunity of the prison population in the Bengal presidency was also remarkable, though the ratios of attacks among the European and native troops were much closer than in Madras. In the Bombay returns the attacks of Dengue are not given for the prisoners, so we have no means of ascertaining whether they escaped the disease to anything like the same extent as in Bengal and Madras.

With respect to the circumstances connected with the recent spread of Dengue in India, and elsewhere, Dr. Charles, of the Calcutta Medical College, while regarding the disease as a contagious malady, remarked in 1872:

"I do not attach very much importance to our being able to trace where the first case of dengue in Calcutta came from, as other conditions besides the existence of a previous case of the disease are necessary before the disease can extend as an epidemic. In our ignorance of what these conditions are, we term such conditions *epidemic influence.*

To my mind the important point seems to be that such a widespread atmospheric or other cosmic state existed during the present time as to favour the diffusion of dengue from person to person over Egypt, Arabia, and India." . . . . "As soon as these unknown conditions necessary to allow the existence of an epidemic are over, a long series of years will follow during which sporadic cases of Dengue will occur in Egypt, Arabia, and India, as well as in other places in which it is endemic, and yet the disease will not spread. Under such circumstances we shall have small communities suffering from local outbreaks, single towns, or it may be some parts of towns, more or less disturbed by this most unwelcome visitor, but the pandemic wave which is at present favouring the universal diffusion of the disease over thousands of miles being wanting, Dengue will under the altered conditions spring up and die down within comparatively narrow limits (*Eighth Annual Report of Sanitary Commission with the Government of India*, p. 112.)

Though vaccination is inculcated assiduously by the authorities in various parts of India, its influence in diminishing smallpox is still but limited, and epidemics of that disease spread over the country every few years causing great mortality. In 1867-8 there was a severe epidemic in the Madras Presidency, which declined there in 1869, but in that year embraced the Central Provinces, and Berar, Oudh, the North-western Provinces, and the Punjaub, where also the epidemic declined in 1870. It may be remembered that early in 1870 smallpox became very active in the south of France, and in the course of that year passed northwards, and towards its close showed signs of increased activity in this country, passing into the severe epidemic of 1871-2.

Smallpox was at its minimum over nearly all India in 1870. From that date it increased in the Madras Presidency, year by year, until 1873, when there were 51,872 deaths from it registered, or 1·70 per 1000 of the population. In the Bombay Presidency the increase went on to 1872, when 1·83 per 1000 of the population died of it; but in 1873 the millesimal ratio of deaths was just one third of this, or ·61. Berar, like the neighbouring territory of Bombay reached its highest ratio, 3·83, in 1872, but remained at 3·81 per 1,000 in 1873; while in the Central Provinces, adjoining it, in Oudh, and the North-western Provinces, the deaths from smallpox became more numerous each successive year after 1870, and, in the last two, amounted to 2·15, and 3·15, per 1000 respectively. Until the epidemic pass over it is not possible to define the exact bearing of the provincial manifestations to each other, nor to indicate its future progress; the subject, however, is a most interesting and suggestive one, which the system of registration of deaths now followed in India is likely to throw much light upon.

A very large number of deaths among the civil population are returned under the term fever, but inasmuch as the vast majority of these are registered on the information derived from people unacquainted with the subject, all the authorities seem agreed that no great reliance can be placed on the numbers so recorded. Cholera, smallpox, or bowel complaints, have all sufficiently distinct characters to admit of most persons of ordinary observation distinguishing them after a very limited experience, and hence the returns may be accepted, as far as they go, as representing those diseases with tolerable accuracy, but to distinguish fevers properly so called, from diseases merely presenting febrile symptoms during their course, requires special education, which does not now exist even to a moderate extent, and which cannot be expected to be generally available for many years to come. Dr. Cuningham, however, is inclined to look on the variation in the number of deaths from year to year as affording a fair indication of the fluctuations of fever over the face of the country, especially when the statistics are borne out by the returns of the sickness among the European and native troops, and in the jails. As the registration of deaths becomes more complete as to numbers, fevers, as well as other diseases, will appear to have increased above the average of previous years ; but the fluctuations just referred to may still be detected among the different districts, and when borne out by the military and jail returns, may be deemed sufficiently reliable to show the incidence of fevers at various points, though not precise enough to form the basis of accurate statistical inquiry.

A large portion of the fevers met with in India are of the malarial class, intermittent or remittent in form ; and, where these are common, other descriptions of fever may present more or less of the remittent type, and thus mask their real character. Those who have not had to watch fever, or indeed other diseases, in the tropics, or in a very malarious locality, can have little idea how much they are often obscured by a tendency to alleviation or increase of their symptoms at definite intervals. To this in some measure must be due the tardy recognition of enteric fever, for instance, which now seems not uncommon in India, and which, presumably, was equally common formerly. Notwithstanding the attention which has been drawn to this subject of late years by Dr. Bryden, it may be doubted whether even yet more than a small proportion of the cases which occur be diagnosed during their progress; for, among the European troops in the three Presidencies, while there were 110 deaths from enteric fever in 1872, the admissions during that year were only 221; and in 1873, with 79 deaths, there were only 210 admissions ; while our experience in this country, as to the mor-

tality attending this disease, would lead us to expect a much larger number of cases would have come under treatment. It was doubtful for some time whether enteric fever occurred among the natives, and Dr. Cuningham adverts to the difficulty of recognising the disease with certainty in a person with a dark skin, and the necessity of caution in adopting such a diagnosis without the corroborative evidence afforded by post-mortem examination. In the report for the Punjaub for 1873 three fatal cases in natives are given by Surgeon-Major Johnson, 5th Goodha Regiment, in which the characteristic intestinal lesion was well marked, which shows the possibility of their occurrence beyond doubt; indeed, there was no very clear reason why enteric fever should not affect the dark races of Hindostan as well as those elsewhere; for it has been known for many years that the negro is quite as susceptible of it as the white man. Mr. De Renzy, Sanitary Commissioner of the Punjaub, suggests that as the exciting cause of enteric fever is so generally present, a very small percentage of native children escapes that disease in infancy, and hence when they grow up they are less liable to another attack.

As to the general prevalence of fevers in 1872 the ratio per 1000 among the civil population showed an increase over that in 1871 in the Madras, and still more in the Bombay Presidency. The rise also was very marked in Berar and the Central Provinces, and to a less extent in the North-western Provinces, and the Punjaub. The increase in Madras was chiefly confined to the Ganjam, Vizagapatam, Godavery, Kistna, and Nellore districts, on the east coast, and to Kurnool in the centre of the peninsula. In Bombay it was apparent in Kulladghee, adjoining Kurnool, and was continued northwards in the Satara, Poona, Ahmednuggur, Nassick, and Khandeish districts, which last adjoin Berar. There being no registration for the territories under native rule in the centre of the country, round which these districts are situated, the state of fever there is unknown. In 1873 the deaths from fever were very much less in nearly all the districts mentioned.

The fever became prevalent in the Madras districts, above named, after the setting in of the rains, which were very heavy. Although this has frequently been the case elsewhere, it must not be concluded from these instances that all that is required to ensure the prevalence of malarial fever is merely abundance of rain; experience, both in India and in other countries, has shown not only that the rainfall may have been unusually great, and as far as regards fever, the season healthy, but on the other hand fever may have become epidemic while the rainfall for the period had been much less than common. There

was a widespread and severe outbreak of fever in India, in 1869, embracing the Punjaub, North-western Provinces, Central Provinces, and even Kurrachee at the mouth of the Indus. Some of the sanitary officers, who had under their supervision districts in which there was much irrigation, attributed the prevalence of the disease to that, but Dr. Cuningham showed that in other districts where there was no irrigation, fever prevailed quite as severely, or even more so than in those where that was in full operation. Fever, no doubt, is met with in wet and malarious districts generally, and in ordinary seasons, in greater frequency than in those which are drier, and free from marshy ground; but from time to time it becomes epidemic over a large area, embracing both descriptions of country, and, as mentioned above, it may then prevail to as great or even greater extent, in the latter class of localities than in the former. There must, therefore, be some factor or factors of extensive operation, and variable incidence concerned, such as Dr. Cuningham has shown are necessary to account for the diffusion of cholera or smallpox, and which, as Dr. Clark remarks, in our ignorance of their nature, we term *epidemic influences*, and until we become better acquainted with these, and are willing to interpret the phenomena which occur within their area of activity, in subordination to them, it is hopeless to expect we can emerge from the confusion of opinion which prevails at present on the mode of propagation of epidemics.

VIII.—Leudet's Clinique Medicale.[1]

The author of this volume is the Director and the Professor of Clinical Medicine in the Rouen School of Medicine. He writes from twenty years experience and clinical observation in the large hospital of that city, following upon an "internat" of six years in the hospitals of Paris under the direction of their world-renowned physicians, and a period devoted to travel and study in foreign lands. Yet Dr. Leudet has deemed it necessary to write an apologetic preface, bewailing his exile from the capital, and referring with much humility to his position as a provincial physician deprived of the many advantages derivable from association with the celebrities of the great city, and of the means of research available to investigators whose better fortune has lodged them in Paris.

[1] *Clinique Médicale de l'Hotel Dieu de Rouen.* Par E. Leudet. Paris, 1874.

And, without question, the practitioners of a capital enjoy advantages, social and scientific, not attainable by those of the provinces. Yet they may, as we can testify Dr. Leudet has done, render great service to the advancement of medicine, and need not be ashamed of the work they accomplish. Moreover, as our author points out, physicians of provincial hospitals enjoy greater opportunities of getting a knowledge of the history of their cases, as well as of following out that history in the course of repeated admissions into the local institution. For example, he remarks that, of 800 or 900 patients annually admitted into his department of the Rouen hospital, 150 at least have on an average been previously under his observation. And as he takes notes of all his cases he is consequently able to get a more or less complete history, and to arrive at a comprehensive clinical review of their maladies.

The volume produced is essentially a practical one, made up of cases carefully observed and clearly reported, with comments on pathology and treatment. The author attempts no systematic descriptions of maladies, but presents a selection of cases, mostly illustrative of the less usual forms of disease. Some of the essays have heretofore appeared in the medical journals; these he has taken the opportunity to revise. Altogether seventeen subjects are more or less fully discussed, some of them at considerable length; and it is satisfactory to observe that he has in his observations referred to the contributions both of German and of English physicians. Among the pathological questions treated are—suppurative pylephlebitis consecutive to diseases of the liver and biliary ducts; the pathogeny of cerebral accidents in acute articular rheumatism; annular ulcerations and strictures of the small intestines; encysted pleurisies and their terminations; chronic meningitis and its influence in the production of polyuria; congestion of spinal cord following falls and strains; the curability of ascites, and the advantages of capillary puncture in the treatment of that disease. Long chapters also are given illustrating the clinical history of entozoa, of glycosuria, and of the influence of alcoholic drinks in producing inflammations of the liver. The conclusions arrived at on the review of the clinical histories detailed are marked by sound practical reflections, and are very usefully summarised at the end of each essay. Several such summaries might profitably be quoted, but it must suffice to give two or three by way of example.

The cerebral disturbances occurring in the course of acute rheumatism have not been so fully elucidated as they demand. According to Leudet's experience they are due to several causes. In the first place we must distinguish from them intercurrent

nervous symptoms, dependent on an anterior neuropathic state, such, for instance, as epileptic and hysteric crises, provoked by the rheumatism, as they might equally be by any other acute malady. Other like intercurrent conditions obtain in the case of previously produced alcoholic saturation, of a scorbutic state, and of a hæmorrhagic diathesis. The brain and its membranes are not often attacked by a true phlegmasia, although now and then the meninges exhibit traces of inflammation. Congestions, or otherwise anæmia, of the brain and of the upper portion of the cord are the most frequent changes observed. The base of the brain, the superior portion of the cord, the areolar tissue outside the dura mater, the meninges, and the cord itself, may be the seat of true inflammation. This variety of congestion is tolerably frequent. Lesions of the heart, of the pericardium and endocardium, and of the blood, frequently provoke cerebral disturbances in the course of acute rheumatism. Among such are pericarditis, endocarditis, myocarditis, clots of spontaneous formation within the heart, the hæmorrhagic diathesis and purulent infection. Likewise rheumatic nephritis has a share in their production. The forms of cerebral rheumatism are very variable, a special variety occurs in infancy, and has been well studied by H. Roger.

The disordered cerebral phenomena may appear isolated, or else may be united in groups. The brusque onset of comatose symptoms or of delirium, causing rapid death, is commonly associated with cardiac lesion: the same probably holds true with regard to the maniacal and melancholic forms of attack. Convulsions seem generally due to renal complication. As to treatment, its indications must be sought in an acquaintance with the causes of the intercurrent cerebral disturbances.

The author relates two instances of pleurisy with displacement of the spleen—a condition respecting which conflicting views have been held. Splenic engorgement and enlargement, common in other inflammations, is a concomitant condition of the displacement in question. Another factor concerned in the matter is a relaxed state of the attachments of the spleen; but there is no direct relation between the degree of displacement and the amount of effusion in the pleural sac.

The clinique of chronic meningitis in relation to its influence in producing polyuria exibits no more than the occasional occurrence of this disorder where meningitis exists, and does not suffice to establish a connection between the two morbid conditions in the way of cause and effect; although this much appears, that an aggravation of the symptoms of meningeal inflammation is attended by an increased flow of urine. It would, nevertheless, be vain to ignore the accumulated evidence,

derived from a host of observers, of a correlation between brain lesions and even passing cerebral disorder, and the occurrence of polyuria, and, still more, even of glycosuria. We have, however, yet to learn to discriminate those lesions of the nerve-centres which do provoke polyuria from those that do not. We have still to unravel the various causes of polyuria, and, we may add, of glycosuria also.

The excessive discharge of simple urine was, in Leudet's cases, unaccompanied by emaciation and by cachexia—a circumstance not always predicable in that functional disturbance.

The examination of his cases of chronic meningitis leads Leudet to results differing in no respect from those arrived at by other observers. Like others he notes occasional absence of intellectual disorder, even where the motor and sensitive nerve-functions are intensely deranged. And he remarks that at times the disturbed nervous phenomena manifest themselves in the splanchnic, or in the vaso-motor nerves, and give rise to simultaneous or alternate perversions of mobility and sensibility, or to herpetic eruptions in the course of the nerves.

M. Leudet's experience of cases of entozoa has been considerable. The *Tænia armata* is the only species of *Tænia* he has met with at Rouen; but 38 cases have occurred to him, 35 of which were among private practice. *Cysticerci* have been met with by him in the brain in 5 instances, in the heart in 1, and in the muscles in 5.

Hydatids he has found in the brain in 3 cases; in the pelvis and broad ligament in 2; in the liver in 24, and in the lungs in 3. The youngest sufferer with *Tænia* noticed by him was a child five years old. In his experience the labouring classes suffer less frequently than those of higher social position. He refers despondently to the results of treatment of tape-worm, but does not inform us what drugs he has employed. His recorded cases are well worthy of study, particularly those of hydatid disease; but his general remarks on the natural history and the migration of entozoa show a want of acquaintance with some of the most recent researches on such matters, and we regret to find that he is not acquainted with the very lucid and conclusive researches of Spencer Cobbold.

Lead-poisoning is common in Normandy, and much of it is due to the cider-drinking habits of the inhabitants. The traditional belief is that the poisoning, recognised by the public mainly in the phenomena of colic, is due to the acidity or other unwholesome condition of the cider, and is consequently a vegetable colic. But such a variety of colic is not known to Leudet; who, on the other hand, finds lead colic common, together with the severer consequences of lead-poisoning. He

concurs with Dr. Todd in the recognition of lead-gout, and states that chronic parenchymatous nephritis is common among the sufferers from lead. Cardiac lesion, usually in the form of simple hypertrophy of the left ventricle is another consequence of lead-poisoning. Lead at one time had the reputation of preventing pulmonary tuberculosis, but, as Leudet shows, it rested on no satisfactory evidence.

On these conclusions we may remark that the causal connection between lead-poisoning and cardiac disease, in the shape of hypertrophy of the left ventricle, first suggested by Duroziez, and now much more confidently put forward by Leudet, is not, in our opinion, made out. The latter bases the hypothesis mainly on the post-mortem examinations of 24 cases of death among sufferers from lead, 17 of which presented cardiac lesions. But besides these in 8 other such sufferers, who did not succumb, what he terms an anomalatrophy of the heart was met with, thus raising the number of instances of cardiac disease to 25 of a total of 184 cases of lead-poisoning. Of the 17 examples of such lesion, as found after death, 14 consisted in hypertrophy of the left ventricle. In one of the remaining the opposite condition of atrophy was found; but if the tendency of lead is to induce hypertrophy, the reverse condition can scarcely be attributed to it.

We have seen a great deal of lead-poisoning, although very few fatal cases, and cannot appeal to the positive evidence for or against the coexistence of cardiac disease furnished by examinations after death. But in the many instances we have had under treatment we have found no such prevalence of heart disease as Leudet would make out. This negative evidence, however, cannot have great weight assigned to it, since, as Leudet asserts, the muscular contractions of the heart are enfeebled, and the force of the pulse is likewise considerably diminished, as proved by the sphygmograph. In other words, the signs of hypertrophy are wanting during life; and no one consequently can gainsay the inference of Leudet that, although he can get proof of only 22 examples of hypertrophy of the heart in 184 cases, yet it may be assumed the lesion was overlooked in a considerable number. Nevertheless, with such facts as he advances, we would hint that a cardiac change in one of eight cases of lead-poisoning is a proportion scarcely sufficient to establish an immediate pathological relation between that cause of sickness and cardiac hypertrophy; and that, besides, we need know many particulars of the history of the cases exhibiting the two conditions, so that recognised concurrent causes of heart disease might be eliminated, before we can accept even

the figures furnished and deduce from them stable statistical facts.

On the assumed correlation between lead-poisoning and renal disease we must also express much doubt. At least we do not find evidence of it during life. The lesion almost always noted by those who have made autopsies is parenchymatous inflammation, leading to contracted granular kidney. When we look to Leudet's experience we find only two of the 24 cases of death from lead attributed to "albuminous nephritis," although, indeed, at a subsequent page, he tells us that of the 17 individuals who exhibited heart disease after death, 10 of them also presented atrophy of the kidneys with granulations of the surface of the parenchyma. Hence all the evidence Leudet has to bring forward to establish his position, that lead-poisoning and renal disease are related as cause and effect, appears to be that in 12 of 184 cases of lead intoxication there was parenchymatous nephritis; for he makes no mention of the signs of this lesion among the 160 patients who escaped his pathological investigations. In our humble opinion, therefore, the relation assumed cannot be regarded as proven; and more particularly when we have the facts before us that the victims of lead-poisoning at Rouen belong chiefly to the class of house-painters, and that alcoholic drinks are much abused by the working classes of that city, who, in other respects, fare very badly as to diet.

The curability of ascites forms the subject of one of the longest chapters in the book, and it is certainly a topic of great interest and well deserving full consideration. Yet, admitting the value of his cases clinically as exponents of the history, the causes, course, concomitant features, and pathological characters of the malady, we must confess to disappointment when we examine them for the purpose of discovering a foundation whereon to base a truly curative treatment.

Of idiopathic ascites he has no experience, and believes that the presumed instances of this affection were examples of pathological states unrecognised during life. In his own practice cases have occurred seemingly referable to such a variety, but the opportunity of a post-mortem examination has been fatal to the supposition by proving the existence of some serious organic lesion of one or other organ.

Peritoneal effusion in young children appears to be in a large proportion of cases curable, but even cases of this sort Leudet cannot admit as furnishing evidence of idiopathic ascites. He narrates a case of such ascites in a scrofulous boy, four years of age, appearing after an attack of hooping-cough. Cod-liver oil and iodide of iron were administered and the dropsy

rapidly disappeared; but when this had happened signs of hip disease came on, which in its turn yielded to treatment. In this patient Leudet concludes that there was temporary tuberculosis either of the lymphatic glands of the belly, or of the peritoneum.

The next two cases narrated are connected with tubercular disease and happened in adults. In the first the ascites accompanied tuberculosis of the lungs and right pleuritic effusion. Under the influence of iodide of potassium and of iodine applied externally the effusion both in the pleura and abdomen vanished, and in course of time the patient recovered. The dropsy in this instance Leudet attributes to a subinflammatory action in connection with the onset of the tuberculosis of the lungs, and probably itself due to the presence of tubercular matter which does not advance beyond a first stage, or one in which it only produces irritation of the tissue in which it is deposited. The second case described agrees in its principal features with the foregoing.

A further example of the spontaneous disappearance of ascites is given in the case of a man who for years had urethral stricture and chronic double pyelonephritis consequent upon it. The ascites did not appear till after the lapse of ten years from the date of urethral troubles, and after lasting nearly six months passed away with an excessive discharge of urine. The man himself subsequently died, seven months after, from an attack of pneumonia, the dropsy having never returned. Here Leudet assigns the ascites to effusion consequent on a cachectic state and a subacute inflammation of the peritoneum, probably connected pathologically with the chronic urethral and renal disease. This form of ascites he calls fibrinous ascites. He likens this case last mentioned to those intra-peritoneal effusions sometimes met with in lying-in women and in instances of some affections of the abdominal viscera, an example of which he quotes, where the autopsy revealed chronic gastritis with considerable hypertrophy of the coats of the stomach at the pyloric end, hæmorrhagic effusion in the peritoneum, and recent false membranes on the intestinal convolutions. The liver, spleen, kidneys, and the blood-vessels, were healthy.

The next group includes cases of heart disease accompanied by general dropsy and ascites, but their examination proves no more than that the ascites may sometimes pass away for a time; that tapping, if not long delayed, may be usefully employed, and its repetition be borne many times, not only without injury, but with much relief to the patient. Leudet would go further, and,—from the appearances of fibrinous bands and adhesions found within the abdomen of two patients who unfortunately

died and so deprived him of the opportunity of adducing positive evidence of the truth of his opinion—contend that ascites associated with heart disease and anasarca is curable by a subinflammatory state of the peritoneum. The author's hopefulness of cure in the ascites of spirit drinkers with cirrhosis of liver is equally remarkable. His patients die, but not before, to his mind, they exhibit signs of approaching cure of their ascites by the agency of a subacute peritonitis with fibrinous effusion.

He concludes this chapter by a notice of a case of ascites in a rachitic female suffering with syphilis, hypertrophy of liver, albuminuria and iritis. By means of calomel and iodide of potassium the ascites and albuminuria were removed and her general condition so greatly improved that she was able to be discharged. This example of the cure of ascites dependent on syphilitic disease of the liver does not stand alone, but is confirmatory of the experience of others.

As a rider to this chapter on ascites, Leudet introduces a short one detailing two cases in which he practised capillary puncture through an umbilical sac communicating with the peritoneum, for the purpose of curing the dropsy. In the first case tapping had been twice previously resorted to and effected by a large trocar and canula, but was followed by severe prostration on each occasion. On the next occasion that the evacuation of the dropsical fluid became necessary Leudet tried puncture with a fine capillary trocar, and was very successful both in the relief afforded and in the avoidance of all the unpleasant symptoms that had ensued after ordinary paracentesis. This capillary tapping he continued to practise over a period of thirteen months, when the patient succumbed to the general anasarca consequent upon a diseased heart, cirrhosis of the liver, and glycosuria. His other case was also a fatal one from heart disease, but the same mode of practice gave great relief to the ascites during the seven months it was carried on.

A few extracts may be made from the last chapter but one of this instructive volume. It is on the question, often debated, of the influence of alcoholic excess on the production of pulmonary tuberculosis. In the course of twenty years and among 15,000 patients, Leudet asserts that he has not met with more than thirty-seven cases of pulmonary tuberculosis among those addicted to alcoholic excess and presenting signs of alcoholic dyscrasia. Moreover, from a tabulated statement of his cases of tubercular spirit drinkers, he concludes that alcoholism does not manifestly predispose to phthisis; and, further on, he remarks that the visceral lesions due to alcoholic drinks, while they, on the one hand, do not hinder the development of tuberculosis, on the other do not favour its production. In like manner, in his

experience, miliary tuberculosis cannot be attributed to the prolonged abuse of spirituous liquors. On the contrary, tuberculosis may be chronic in the victims of spirit drinking. But alcoholism multiplies intestinal accidents in tubercular disease, in the form of hæmorrhage and of peritoneal mischief, a result of the coexistence of amyloid and fatty degenerations of the liver and kidneys.

The space we have given to this review indicates our appreciation of Leudet's volume of clinical medicine as an honest attempt to portray disease, particularly in some of its rarer forms, in a practical aspect, and to advance our knowledge as well of pathology and pathological anatomy as of treatment.

IX.—Marey on Animal Mechanism.[1]

It cannot be expected that the reviewer of a book like this of Marey's on animal mechanism should be in a position to criticise experiments which he has heard of for the first time in his life—or even be in a position to repeat the experiments for the mere sake of finding out mistakes either in details or results. Instead of being a critic the reviewer is reduced to the more pleasant position of a student acquiring valuable information from the pen of a master of experimental physics. The pathway of research of the present day is, if more difficult to follow, more pregnant with beneficial results to mankind at large. The study of the movements of animals is not one of recent date, but probably greater progress has been made in this branch of science during the last two centuries than previously. Since the time of the first John Bornoulli the action of muscles has been a constantly recurring subject of research to Continental and English anatomists. But progress has been of exceedingly slow growth, and not any more accurate for its tardiness. Little more than a hundred years ago we find a learned professor of anatomy writing as follows:—"The particular mechanism or immediate cause of muscular action has very much tortured the brains of many philosophers. The extreme delicacy of the texture of a moving fibre, and a great number of phenomena, some of them very obvious, which have not been attended to, have hitherto prevented the discovery of this mystery." He shows that muscular fibre had been supposed to be *spongy, vascular, vesicular, contorted, elastic,* &c. And concerning the concurrence of different fluids with the supposed structure of the

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[1] *Animal Mechanism: a Treatise on Terrestrial and Aërial Locomotion.* By E. J. Marey.

fibre, systems have even been founded wholly on the spring or elasticity of the solid parts of which a muscle is composed." Further, the same author remarks, "Till some lucky discovery is made, what can hitherto be gathered from the structure, confirmation, and action of the muscles, is, that their strength depends on the number of their fleshy fibres, and the extent of their action on the length of these fibres." This is a fair summary of that which was known to anatomists a hundred years ago. The next advance made on the knowledge of muscle tissue is due to the microscope; and although not absolutely—yet almost so, this instrument has taught us structure only. The muscular wave—the zig-zag of Magendie—had never been accurately defined before the researches of Prevost and Dumas. The same authors determined a long doubted question, namely, whether contracting muscle was increased or diminished in regard to volume. Borelli maintained that there was an augmentation; Glisson maintained and illustrated by experiment the contrary.[1] Others continued these experiments with variable results—that of Barzoletti satisfying Magendie that "the volume of the muscles changes not during contraction, and whether it did or not was not of much consequence."

Modern researches have not only determined this their disputed point, but have materially extended our knowledge of the physical, chemical, and electrical properties of muscles. This knowledge has been arrived at by actual experiment. Helmholtz well observes that "we of the present day have already sufficient insight to know that the laws of nature are not things which we can evolve by any speculative method. On the contrary, we have to test them by repeated observation or experiment, in constant new cases, under ever-varying circumstances; and in proportion only as they hold good under a constantly increasing change of conditions, in a constantly increasing number of cases, and with greater delicacy in the means of observation, does our confidence in their trustworthiness rise." No one has contributed more to the science of fact than M. Marey. Long known and long respected as an able, talented, and patient searcher after truth—truth based upon carefully conducted experiments—he has boldly entered into intricate fields of investigation, which frightened more timid would-be discoverers—and succeeded in elucidating by his Herculean industry, mechanical skill, and undaunted perseverance, facts in substitution of what previously were mere conjectures at the best. But he has advanced our knowledge in many other respects, and in no one, probably, more than in giving to the world of English readers a book so simply yet so ably written, embodying, as it does, much that it is necessary to know concerning animal mechanics.

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[1] Magendie, 'Compendium of Physiology.'

The introduction of instruments—previously exclusively used in the science of meteorology—modified to suit altered conditions, into the physiological laboratory, marked a new era in scientific research. Much that previously it was impossible to determine can, by the aid of delicate instruments, be demonstrated. These instruments have been introduced into physiology by Volkman, Ludwig, Helmholtz, and Marey, and are used by almost every practical teacher of physiology in the schools of this and other countries.

In the chapter on animal motion one of these instruments, "the myograph," is described and figured, and its function clearly explained. Tracings are given to show the character of the movements produced when electrical shocks are transmitted to muscles.

One of the most beautiful phenomena in connection with muscle is its wave. The author has given a figure of the appearance presented by a wave in muscular fibre. It is not a decided success. But readers of the book must examine a specimen under the microscope, and then they will understand muscular contraction more readily than from the figure in question.

The chapter on "Harmony between the Organ and the Function" is very interesting. The author shows that there is harmony between the form and function of the muscles. Whenever function varies in analogous muscles it is attended by a corresponding change in the configuration of the muscles. This fact is illustrated by the short sternum and thick pectoral muscles of birds with short wings and the long sternum and thin muscles of birds with long wings, the latter during flight offering proportionately a less area of the whole wing surface than the former. Hence the necessity of greater and more concentrated motor power.

"The comparison of homologous muscles in mammals of different kinds is not less instructive under the aspect in which we are now considering them." No doubt they are instructive enough. But the author naively remarks, "One is often embarrassed in this comparison by the difficulty of recognising the homology."

"The discrepancies are oftentimes so striking that anatomists have described under various names the same muscle in different species." If anatomists had confined themselves to so simple a sin of commission—misleading and perplexing though it is—there would have been very little mischief done to the progress of science; but anatomists have for nearly a century vainly struggled in the pathway of confusion to pair muscles of one with those of the other limb, forgetting that difference in function—whatever may have been the determining conditions of the differences in the two limbs, or how many hundred or thousand generations it may have taken to determine the decided difference in function between the two limbs—is the power or factor which modifies and induces muscle change. Muscle is merely a mechanical agent, acted on by

a superior force; and if the superior force wills that the inferior should move in a certain direction, owing to new and inaccessible wants, and the muscle cannot do it, the superior force is capable of educating the inferior until it can do so. Hence, in a succession of generations, that which the first cultivator of the application of a new direction of force could not do without practice its descendants could easily do, and to a greater degree, even approaching to what may be considered to be a new direction, though an extension virtually of the limited original motion of its ancestor.

The author considers that "in the greater number of cases the homology of muscles is not doubtful." "It is implicitly admitted by the fact of an identical designation being applied to certain muscles in different species." Well, it is evident enough that the muscles which extend and flex the toes in the two limbs are homologous and receive similar names; but where the difficulty practically lies is not in those which are so self-evident as between those muscles present in one limb and not in another, and in the limbs of one animal and not in those of another one. The higher we proceed in the animal scale, the more difficult—in fact, impossible—it becomes to adhere to a law of homology; neither is such a law desirable, even if it could be applied without the aid of an active imagination.

Discussing the easy recognition of the biceps femoris, &c., the author states that "If one can rely on the anatomical plates of Cuvier and of Laurillart, the negro has the perineal insertion of the biceps not so high as in the white man, thus approximating to its position in the ape." If this were the case it would be a fact antagonistic to the theory of evolution. The fact is that in both ape, black and white, the biceps attains a similar point of attachment indirectly. The ischiatic ligament is nothing more than an aborted part of the muscle. Beyond these and a few other minor points of no particular moment, the chapter on organ and function is of great interest.

The chapter on "The Development Theory" is a brief *résumé* of the views held by the two schools, the Creationists and the Evolutionists. The author has evidently a conviction that the old school will in process of time be defeated. The great mistake which both schools fall into is the requirement of definite productions from a given status. The Creationist shelters himself behind the "unassailable"—in the "beginning" creation—out of nothing into perfection. The Evolutionist in the beginning nothing but protoplasm. From this status time and circumstances have produced everything. Mere argument will not advance our knowledge one jot or tittle. And thus no amount of research will ever be able to illustrate the various phases which life has manifested in and on the earth. Nor will it demonstrate to us how—if we are to take protoplasm as a

starting-point—the immense number of different species of plants and animals have attained their present status, and maintained so persistently their characteristic features when perpetuated through such long periods. If animals can select, plants cannot, and the latter equally with the former preserve in the majority of instances isolated characteristics peculiar to their kind. But it must be granted that the Evolutionist has many facts in support of his theory. It is not natural selection. If the latter is a potent agent in modifying, it does not account for all the peculiarities met with in anatomical tissues and the gradual but steady and persistent invasion of nerve substance and nerve force. There is an independent force which modifies tissues, and what that force is, and how that force acts, is the task which present and future scientific observers have to determine. Even Mons. J. Guérin's pithy formula, "*Function makes the organ,*" though a mere reversion of an older, equally as pithy a one, is like its more ancient axiom a diminutive truth shorn of its chief glory, "the necessity" which creates the function. And this again leads to the final cause to be sought, wherefore, or how, the necessity? But speculation beyond the stage of easy appreciation, borne out by facts, is absurd and useless. Let the past remain in its arctic tomb until we can read the present. The very tendons, which M. Marcy has not altogether a very clear idea of—that is, as to their true significance—teach us one of the most instructive and forcible lessons in evolution capable of easy demonstration. It is a lesson which neither the creation nor natural selection theories can explain, and it answers the very question asked by the antagonists of evolution.

The fibrous tissue of tendons, aponeuroses, ligaments, &c., are, in the appendicular parts, aborted remnants of muscle tissue. Tendon is not lost, at all events, as M. Marey thinks, in descending the animal scale; but on the other hand it is gained as we ascend the scale—the higher the animal and the more definite and declared its tendons. Muscle wastes in the inverse ratio to the accession of brain tissue. But natural selection does not satisfactorily account for this irrefutable fact any more than it is able to explain the fragments of muscles perfect in lower animals, the peculiarities of bone, and the progressive massiveness and complexity of nerve tissue, found in higher animals. Everything that we know of seems to exhibit a progressive law of evolution. Whatever the beginning, it was evidently simple in status, simple in material, and simple in organisation—whether by creation, natural selection, or evolution, or a combination of any two or the whole of these forces, is not of vital importance. Organized and unorganized are, as exhibited to us now, complicated enough. But analysis undoubtedly shows us that each and every tissue in both kingdoms have been vastly more simple in texture than they appear to us now.

But M. Marey admits the influence of a force or power, whether natural selection or evolution matters not. "If it be true that every fœtus brings into the world a contorted humerus, it is no less true that this form may be considered as the effect of muscular action accumulated from generation to generation." The latter assertion is not so self-evident as it might be when contrasted with previous assertions as to bone markings and bone ridges, &c. There is decided law in regard to these markings. They are not entirely due to degree of muscle appended to them, any more than the number of sternal ribs depends upon the degree of development of the ventral trunk muscles. The variability of the muscular system in its transit from childhood to old age is not so decided in the human subject. As M. Marey states, "The function of the muscles changes with the different periods of life, and becoming more and more restricted employs continually less contractile fibre," &c. Age cannot account for the invasion of tendon already mentioned, and its differentiation. Moreover, it is impossible to explain how nerve force can split tendon from its distal end, and split it so gradually that in different beings every stage of a muscle may be seen, from a short distal differentiation of a tendon to its complete and perfect isolation from the parent mass as a distinct and independent muscle. The learned Borelli says, "It is worthy of admiration that in so great a variety of motions, as running, leaping, and dancing, nature's laws of equilibration should always be observed; and when neglected or wilfully transgressed, that the body must necessarily and immediately tumble down." Locomotion, especially "terrestrial," has long been a subject of interest.

The brief description given of walking, p. 112, embodying some of the analyses of M. G. Carlet, does not materially differ from that of Magendie—in fact, there is little beyond difference in words. But M. Marey's description of his experimental shoe and registering apparatus, to illustrate pressure on the ground and its intensity, is as intelligible as it is unanswerable and illustrative of his great mechanical genius.

The modes of progression used by man and the paces of the horse are now no longer subjects of conjecture, but of absolute knowledge. The author truly remarks, "There is scarcely any branch of mechanics which has given rise to more labour and greater controversy than the question of the paces of the horse." Again, "Any one who proposed at the present time to write a treatise on the paces of the horse would have to discuss many different opinions put forward by a great number of authors."

There seems to be a certain unanimity amongst authors as to the movements of the limbs in the simpler paces of the horse, such as in walking and ambling. Various contrivances have been resorted

to to interpret that which was almost impossible by unaided observation.

Marey has succeeded by the direct application of ingenious registering apparatus in producing a series of experiments alike interesting to the physiologist and artist. From these experiments the former "can derive the expression of the duration, actions, and reactions of each pace, the energy and duration of each movement, and the rhythm of their succession;" while the latter (the artist) knows "exactly the attitude which corresponds with each movement, and is thus enabled to represent it faithfully with the various poses which characterise it."

These experiments have been carried out to verify and amplify the principles already known and established by Vincent and Goiffon, Baron Curnien, &c.

M. Marey has not discussed aquatic locomotion. His reason for not doing so is that the recent experiments of Mons. Ciotti have thrown great light on the propulsive action of the tails of fishes; not that they have overthrown the theory held ever since the time of Borelli concerning the mechanism of swimming, but they have approached the question in another manner, that of synthetic reproduction of this phenomenon. This method will certainly permit us to determine with a precision hitherto unknown both the motive work and resistant work in aquatic locomotion. It will therefore be advisable to wait for the results of experiments which are now being made, and which will be of equal service both to mechanicians and to physiologists.

It is evident, even to the most superficial observer, that whilst terrestrial locomotion can be readily studied, ærial locomotion presents a series of formidable difficulties to be overcome before trustworthy knowledge, based upon actual experiment, can be obtained. In the former there is a fixed and tangible surface of reaction, whereas in the latter there is merely an attenuated and shifting medium, to which the organs of locomotion impart a certain volume of moving force in a direction contrary to that in which the animal moves. Hence it is only in proportion to the rapidity with which it is displaced that the air resists the impulse of the wing.

In studying, therefore, the phenomena of flight of insects greater ingenuity and delicacy of manipulation are required than in demonstrating the paces of a horse. The questions to be determined, according to Marey, are—

1. What is the frequency of the movements of the wing of insects?

2. What are the successive positions which the wing occupies during its complete revolution?

3. How is the motor force which sustains and transports the body of the animal developed?

These three questions the author proceeds to answer, illustrating his explanations by a series of beautiful and admirably conducted experiments. On reading the chapter on "Flight of Insects" we find ourselves more and more impressed with the already established conviction that we are in the hands of one thoroughly master of his subject. The method of determining the frequency of the wing movements, the influences which modify their frequency, the synchronism of the action of the two wings, the changes of plane, &c. are honestly demonstrated by, what seems to us, experiments as accurate as they are unanswerable.

It has long been an established fact that the frequency of the movements of the wings varied according to species. But it had not been determined with the same precision as by M. Marey. The graphic is far superior to the old musical note method. "*It enables us to ascertain almost to a single beat the number of movements made per second by an insect's wing.*"

Pettigrew ('Animal Locomotion,' p. 121) says, "The wing of the blowfly is said to make 300 strokes per second," &c. "Now, it appears to me that muscles to contract at the rate of 18,000 times in the minute would be exhausted in a very few seconds," &c. "I am therefore disposed to believe that the number of contractions made by the thoracic muscles of insects has been greatly overstated; the high speed at which the wing is made to vibrate being due less to the separate and sudden contractions of the muscles at its root than to the fact that the speed of the different parts of the wing is increased in a direct ratio as the several parts are removed from the driving point." Marey says, "These exceedingly complicated movements (*of the wings*) would induce us to suppose that there exists in insects a very complex muscular apparatus, but anatomy does not reveal to us muscles capable of giving rise to all these movements," &c. He explains at length his reasons for the assertion that "*an upward and downward motion given by the muscles is sufficient to produce all these successive acts, so well co-ordinated with each other; the resistance of the air effecting all the other movements.*"

This brings us to the consideration of the following passage, p. 187:—"*We thought that we had been the first* to point out the form of the trajectory of the wing of the insect, but Dr. J. B. Pettigrew, an English author, informs us that he had already mentioned this figure-of-8 appearance described by the wing, and had represented it in the plates of his work.[1] It will be seen presently that, notwithstanding this apparent agreement, our theory and that of Dr. Pettigrew differ materially from each other." Again, in describing the propulsive action of the wings of insects, p. 199, Marey

[1] "On the Mechanical Appliances by which Flight is maintained in the Animal Kingdom," 'Trans. Linn. Society,' 1867.

says, "Each stroke of the wing acts on the air obliquely, and neutralizes its resistance, so that a horizontal force results which impels the insect forwards. This resultant acts in the descent of the wing, as well as in its upward movement, so that each part of the oscillation of the wing has an action favorable to the propulsion of the animal." Further, contrasting the direction of movements in the trajectory of the wing, Marey shows that the movement is in opposite directions in the two branches of the 8. Pettigrew makes them run in the same direction, and, "in order to explain the form which he assigns to this trajectory, Dr. Pettigrew admits that in its passage from right to left the wing describes by its thicker edge the thick branch of the 8, and the thin branch by its narrow edge. The crossing of the 8, therefore, would be formed by a complete reversal of the plane of the wing during one of the phases of its revolution. In fact, the author seems to perceive in this reversal of the plane an action similar to that of a screw, of which the air would form the nut."

Now, it must be granted that as far as the mere discovery of the figure-of-8 movement is concerned Dr. Pettigrew is entitled to priority of discovery. But Marey's independent discovery and explanation of the figure-of-8 movement of the insect's wing is as different from that of Pettigrew's as the second law of Kepler (that the planetary orbits are elliptical, having the sun for their common focus) is to that of the abstract theory of the curve of the old Greek geometers. But Dr. Pettigrew is not of this opinion; at all events was not last year. Replying to a criticism on his book by Ruskin ('English Mechanic,' Feb. 13th, 1874), Pettigrew summarises his views as follows:—"Quadrupeds walk, and fishes swim, and insects, bats, and birds fly by figure-of-8 movements." "The flipper of the sea bear, the swimming wing of the penguin, and the wing of the insect, bat, and bird, are screws structurally, and resemble the blade of an ordinary screw propeller."

"Those organs are screws functionally, from their twisting and untwisting, and from their rotating in the direction of their length, when they are made to oscillate.

"They have a reciprocating action, and reverse their planes more or less completely at every stroke.

"The wing describes a figure-of-8 track in space when the flying animal is artificially fixed," &c.

There can be no question of the fact that either Marey is right and Pettigrew wrong, or the reverse. From a careful contrast of the two explanations and from other evidence it seems to us that Dr. Pettigrew's views are not in accordance with fact, and he would do well either to repeat his own and Marey's experiments or gracefully allow to Marey the honour to which he seems to us so justly entitled. Dr. Pettigrew cannot honorably persist in the statement

that Marey admitted the priority of his (Dr. Pettigrew's) discovery. That statement was made under a mistaken idea of the general bearing of Dr. Pettigrew's discovery; Marey now repudiates it, and he has shown that there are just and decided grounds for so doing.

Thus far Marey's researches on flight—and difficult and tedious these researches have been—verify more or less completely the theory of flight propounded by Borelli. Dr. Pettigrew maintains in opposition to several observers whom he himself quotes in his work, and also to Marey, "that a natural wing, or a properly constructed artificial one, cannot be depressed either vertically downwards or downwards and backwards. It will of necessity descend downwards and forwards in a curve. This arises from its being flexible and elastic throughout, and especially from its being carefully graduated as regards thickness, the tip being thinner and more elastic than the root, and the posterior margin than the anterior."

Again, "The flat surfaces of the wings are consequently made to strike downwards and forwards, as they in this manner act as kites to the falling body, which they bear, or tend to bear, upwards and forwards."

"In no instance, however, unless when the bird is everted and flying downwards, is the posterior margin of the wing on a higher level than the anterior one."

Dr. Pettigrew has been severely taken to task more than once for the above statements. Still, he appears to be remarkably confident in his own views, and even repeats them after criticism. It would be premature at the present time to decide upon the merits of the two observers, Marey and Pettigrew.

We prefer to wait until Marey's further researches on the important subject of flight have been published. In the meantime Dr. Pettigrew may probably deem it necessary either to bring forward more decisive evidence in support of his statements, or show more conclusively than he has yet done that the views of his opponents are contrary to facts.

M. Marey's book is one that will be widely read. If it is not the pioneer into long unexplored fields of research, it is the guide to them. We have no doubt that it will be the means of stimulating many enthusiastic workers to greater discoveries than even M. Marey has made. We wish M. Marey as great and as well-deserved success in his future as in his past labours.

X.—Recent Works on Medical Jurisprudence.[1]

The list of works we have placed at the head of this article indicates pretty clearly in what direction modern medical jurisprudence has made its chief advances. It will be observed that most of the above volumes, except those relating exclusively to toxicology, are new editions of old and standard works. It is in toxicology, then, that medical jurisprudence has chiefly progressed during the last decade. Nor can this be wondered at. When it is considered what a host of new remedies are being yearly added to our list of materia medica—many of them containing principles of great potency for good or evil—and the astonishing advances made by modern organic chemistry, mainly in the direction of the discovery of new compounds, it can hardly be an object of wonder that the studies of medical jurists have been much devoted to toxicology. It is true, nevertheless, that considerable advances have been made in other branches of the science, but not to a commensurate extent.

In order to verify our remarks let any one take up and compare the third edition of Guy's 'Principles of Forensic Medicine' with the fourth edition, now before us, brought out jointly by Drs. Guy and Ferrier. We advise a comparison of the two most recent editions of this work, because it may fairly be considered to be the usual text-book read by English students, just as the 'Principles and Practice' of Dr. Taylor's is the standard work of reference among lawyers. Both treatises, then, being the writings of able and practical men, may be fairly taken to represent adequately the assured facts and assured opinions of medical jurists of the present day. It will be found that the first two parts of the last edition of Dr. Guy's manual are substantially the same as those of the previous editions, whilst the third part of the new edition, treating on toxicology, is much enriched, and, as we learn from the preface, this has been done by Dr. Ferrier. It is true that a chapter on personal identity is

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[1] 1. *The Principles and Practice of Medical Jurisprudence.* By Alfred Swayne Taylor, M.D., F.R.S. Second edition. London, 1873.

2. *Wharton Stillé's Medical Jurisprudence.* Third edition. Philadelphia, 1873.

3. *Principles of Forensic Medicine.* Fourth edition. By William A. Guy, M.B., F.R.S., and David Ferrier, M.D. London, 1875.

4. *A Manual of Toxicology.* By John J. Reese, M.D. Philadelphia and London, 1874.

5. *Die Gerichtlich-chemische Ernmittelung von Giften.* Von Dr. Georg Dragendorff. St. Petersburg, 1868.

6. *Beiträge zur Gerichtlichen Chemie einzelner Organischer Gifte.* Ibid.

7. *Manuel de Toxicologie.* Par Dragendorff, traduit par E. Ritter. Paris, 1873.

8. *Beiträge zur Gerichtlichen Chemie einzelner Organischer Gifte.* Von Dr. Dragendorff. Petersburg, 1872.

9. *Guy's Hospital Reports.*

added, *apropos* of the Tichborne case; but this is interesting, not on account of new researches or discoveries, but because old and well-known principles are applied in the elucidation of a special and highly complicated case.

Not many years have elapsed since toxicology was nearly exclusively confined to the consideration of inorganic or mineral substances, and rarely travelled—at all events as a science—further than these and to a few organic, or, as they are now generally termed, carbon compounds, such as prussic acid and morphine. The use of strychnine in a few cases which became *causes célèbres* turned the attention of men of science to the organic principles known as alkaloids; and, still more recently, the discovery and application in the industrial arts of aniline, and the multitudinous hosts of products derived from, or allied to, that substance, have greatly extended our knowledge of nitrogenous organic bodies. Moreover, the increased study, especially on the Continent, of the physiological action of poisons and remedies generally, has vastly extended the area of toxicology. Nor has the study of the chemical reactions of organic bodies been neglected; indeed, 550 octavo pages of the French edition of Dragendorff's 'Manual of Toxicology' are nearly exclusively devoted to chemical analysis, and of these about 100 are taken up with the subject of testing for the alkaloids. Such a vast array of matter indicates activity of research, more, perhaps, than exactitude of knowledge.

It is evident that our Continental chemists are far ahead of our English brethren in their experimental knowledge of the alkaloids. Dr. Guy treats, indeed, of but some seven or eight of the alkaloids, and Dr. Reese of about the same number; whilst Dr. Dragendorff gives us the chemical characteristics of about thirty of those interesting principles, all of which may be met with in toxicological investigations, and his descriptions are much fuller and more exact than those of the English author. With true insular prejudice British chemists in great measure despise the alkaloids, and for this reason, apparently, that their chemical constitution is to us still so much a mystery. Indeed, one recent systematic English writer of a treatise on organic chemistry has entirely omitted all mention of the alkaloids from his work, evidently because their relation to other well-understood carbon compounds has not been satisfactorily established. And thus it happens that, with a few brilliant exceptions, little has been done in this country to advance our knowledge of the chemistry of the alkaloids.

But though our knowledge of the chemical relations and constitution of the alkaloids is still immature, their reactions are, fortunately for the practical toxicologist, better known, and they are as a class marked off from other carbon compounds by their multitudinous so-called colour reactions, which, if not affording evidence for their

detection of the highest order of certainty, may, if taken in conjunction with their physiological actions, afford a body of evidence sufficient to enable us to draw sound conclusions as to the presence or absence of a given poison. It would be out of place here to enter into details as to chemistry proper; but the student of toxicology will find the fullest and latest information as to toxicological analysis in the French edition of Dragendorff's manual, which, we may add, embodies nearly all that is contained in the two volumes of Dragendorff, written in the German language, whose titles we have placed at the head of this article.

Physiological research has during the last decade done much to extend our knowledge of the action of poisons; and our German brethren have been very active in pursuing this branch of scientific investigation; but even on this side of the Channel we have not been idle, and we have among us energetic and able workers in this field. We only hope that their labours may not be abridged by the persistent attacks of the Society for the Prevention of Cruelty to Animals; for we believe that pain inflicted upon animals by physiological experiments cannot for one moment be placed in comparison with the good resulting from the discoveries which are made through this mode of investigation.

Sufficient attention has perhaps not been given to the application of physiological taste for organic poisons, although some ten years ago the attention of the profession was prominently directed to this promising line of search. Indeed, in 1856, Dr. Marshall Hall first suggested the tetanic spasms produced in frogs by strychnine as a means of detecting that poison; and in 1864 on the occasion of the trial of M. de la Pommerais for the murder of Madame de Pauw, physiological tests for digitaline were chiefly relied on as evidence of the administration of digitaline. In 1865 Drs. Fagge and Stevenson communicated a long experimental research on this subject to the Royal Society, their object being to show that physiological tests may be made independent of any relation between the action of poisons on man and on the lower vertebrata. They endeavoured to avoid altogether the difficult question of identity of action of poisons upon men and the lower animals upon which all physiological evidence in cases of poisoning had up to that time been based; and it appeared to them sufficient that the action of the substance supposed to contain the poison on the animal experimented on be identical with the known effects of that poison upon the *same kind of animal*, and that these effects be capable of being produced by no other agent, or only by a limited number of such agents. And, shortly after, Prof. Penny and Dr. Adams, in describing the methods pursued by them in their search for poisons in the case of Pritchard, also arrived at the conclusion that it was of little moment that the phenomena manifested in rabbits to which a supposed poisonous

liquid had been administered should differ from those manifested by the human subject when under the influence of the same agent, so long as the toxic action was uniform and characteristic when employed on the same animal. The principles thus clearly laid down do not, nevertheless, appear to have been generally adopted by toxicologists; and we imagine the reason for this lies in the fact noted by Drs. Fagge and Stevenson that a limited group consisting of few poisons may be apparently identical in their action when administered to the lower animals: physiological tests may point to a group of poisons, but fail to differentiate any single poison of the group. Spite of this radical defect physiological tests are of great value and in many cases cannot be dispensed with, since chemical tests, so far as is at present known, are not always competent to determine the presence of some of the organic poisons. Another circumstance militating against, though not absolutely vitiating, the application of physiological tests for poisons is, that extracts made from the gastric contents of the higher animals may, as pointed out by Drs. Fagge and Stevenson, under certain circumstances exercise a toxic influence upon the lower animals, as *e.g.* frogs, animals which are above all others most available for the purposes of the toxicologist.

For a reason we have already alluded to, namely, that it is the most concise English manual of medical jurisprudence, and is also, as we believe, the one most generally read by medical students, we shall chiefly confine our criticisms to the work of Drs. Guy and Ferrier; only noticing the other works named at the head of this article where special mention appears requisite.

The manual of these authors does indeed form a compact and handy book of reference, and in it the ordinary medical man will find all that is necessary to guide him in his practice; though lawyers and medical jurists will, we imagine, greatly prefer the larger and more ponderous volumes of Dr. Taylor. The great defects of Dr. Guy's manual are that accuracy is occasionally sacrificed for the sake of brevity; that statistical details are given with undue minuteness, and valuable space is thus sacrificed to the exclusion of valuable matter; and that the book has a certain hardness and dryness of style which renders it somewhat repulsive reading, spite of the interesting nature of the subject. We will endeavour to illustrate these statements by examples taken from the work itself.

The section in Dr. Guy's 'Principles' which treats of the detection of spots of blood is not altogether satisfactory. Its chemistry is obscure and not altogether precise and accurate. Thus, in order to discriminate between blood pigment and the red sulphocyanate of iron it is stated that the latter gives "a white precipitate of oxide of iron" when treated with ammonia, though no white

hydrate is known to chemists. Again, the mode of applying the guaiacum test is given without any of the precautions clearly laid down by Dr. Taylor, and usually adopted by the medical jurist. Moreover, it is stated that in the microscopical examination of a blood-stain a quarter-inch object-glass is as high a power as it is necessary, "or, perhaps, desirable," to use; and it is said that the corpuscles can be well seen with a good half inch. We imagine that few experts would nowadays be content with the use of an object-glass of less focal length than an eighth of an inch, and most microscopists would prefer to use one of still greater magnifying power. Indeed, some go so far as to use immersion lenses of $\frac{1}{25}$, and even $\frac{1}{50}$ inch focal length; and, it is said, with advantage.

Their treatment of the subject of life insurance is far more satisfactory; and here Dr. Guy is thoroughly at home. Into the compass of nine or ten pages they manage to compress a vast amount of valuable matter—statistical and medical—which the practitioner will find of great service in enabling him to arrive at sound conclusions when investigating lives for insurance purposes. Of course, details cannot be entered into, but the general principles inculcated are excellent. The young practitioner may study this chapter with profit, for it touches upon a branch of practice which he will in vain have sought to gain some knowledge of in the schools. Nevertheless, the statistics with which this chapter is interlarded are at once too brief and unreliable to be of essential service to the practitioner, and would have been well omitted. They are based upon the essentially unreliable returns of the Registrar-General, which, useful as they are in many respects, are valueless in estimating the mortality from such indefinite diseases as "apoplexy" and "heart-disease"—names too often used by medical men in certifying the causes of death without using much discrimination.

In view of the expedition of the exploring ships, the Alert and Discovery, to the arctic regions, the subject of death from cold possesses an absorbing interest; and the best means of averting the effects of low temperatures has received great attention. To the ordinary medical man practising in this country the topic usually presents less of interest than its importance deserves; since the same means must, as a rule, be taken to avert the lesser as well as the greater and lethal effects of cold.

In estimating the effect of cold on the human body, Drs. Guy and Ferrier lay down three modes by which the body is cooled—by cutaneous exhalation, by conduction of the air in contact with it, and by radiation. They say—

"The *cutaneous exhalation* is increased by dry and diminished by moist air. Hence the body parts with its heat more rapidly in a dry atmosphere. On the other hand, the body is cooled by *conduction*, when the air is moist; so that the body is cooled alike by dry

cold air and by cold moist air. Cold humid winds lower the temperature of the body in a very striking degree. A rapid renewal of the air, as in a brisk cold wind, lowers the temperature of the body, both by evaporation and conduction" (p. 341).

Now, we have here no statement of the regulative action of the nerves upon the animal body by which the system is enabled to ward off or resist the effects of cold. Considering the misapprehensions which exist as to the means by which the effects of cold are resisted, such an account would perhaps have not been superfluous. The human body is provided with a set of nerves—the vaso-motor nerves—which regulate the size of the blood-vessels, and this apparatus is constantly in a state of activity without our being conscious of it. Should the media surrounding the body fall in temperature, immediately the above-described apparatus closes more and more the paths by which heat escapes from the body, *i.e.*, by the pores of the skin and cutaneous exhalation. It is literally true, as has been well said, that the cooling of the skin protects the interior of the body for a time from too great loss of heat; and the converse is equally true. Alternations of temperature seem necessary to keep the vaso-motor regulative apparatus in healthy activity; and no small part of the effects of prolonged cold are doubtless due to the loss of this activity.

The extract we have quoted also exhibits a manifest inacquaintance with the real meaning of the term "dryness" as applied to the atmosphere. The term dryness has a purely relative meaning, and is intended to express the state of saturation or non-saturation of the air with moisture. The quantity of moisture which the air is capable of taking up at different temperatures is very various. Thus at 35° Fahr. a cubic foot of air will take up two and a quarter grains of aqueous vapour, at 60° it will take up five and a half grains, and at 80° ten and a quarter grains. Now, a cubic foot of air at 35°, containing two grains of moisture, and a cubic foot at 80°, containing ten grains, may both be said to be equally *dry*, since they are both capable of taking up the same amount of moisture, viz. a quarter of a grain. But in another sense the air at 80° may be said to be much the moister of the two, since it contains five times so much water as the air at 35°. We are not aware whether any experiments have been made to ascertain the relative conductivities for heat of air at different temperatures, when charged with varying proportions of aqueous vapour; but we imagine that the conductivity will be found to be proportionate to the percentage of moisture in the air, and not to its *dryness*, *i.e.* relative saturation with aqueous vapour.

The interesting subject of spontaneous combustion is treated of, as a matter of course, from the sceptical point of view. Orfila was, nevertheless, a believer in the possibility of this form of death.

Few, we imagine, would now-a-days be inclined to adopt Orfila's opinion on the subject. The cases cited by Guy and Ferrier are too ancient to either prove or disprove the statement that spontaneous combustion is a possibility. There are more recent and better reported cases than those cited by the authors we have just named; and it would have been well for them to have given a critical account of the case of Mrs. Warrack detailed by Wharton and Stillé (§ 865).

The remarks of these authors on the subjects are worthy of attentive consideration, and embody a cautious and philosophical view of the question at issue. They say:

"Admitting that the phenomena of spontaneous combustion, so called, are incongruous with the laws of combustion so far as they are known, it does not follow that we should with these chemists (Liebig and Bischoff), reject as unworthy of belief the many curious and authentic facts on record. These may be true, although incorrectly accounted for. Indeed, there are many examples of the spontaneous combustion of organic and inorganic matter, which chemistry is unable satisfactory to explain. But the number of cases now known, amounting to between forty and fifty (some of them, perhaps, indeed fictitious), the uniformity in the description of the phenomena, and often age and habits of the persons attacked, require us to regard them as scientific facts yet unexplained" (§ 867).

The subject is one which certainly cannot, in view of the recorded facts, be dismissed with a sneer.

In treating of the important subject of burns, Dr. Guy adopts the conclusions of Chambert, though, oddly enough, "with modifications suggested by the more certain of the results," not of the later but "of the *earlier* writers." Nevertheless, though the preference is thus given to the earlier rather than to the later authorities, his descriptions are generally good, and his conclusions such as are accepted by most modern medical jurists. We note, however, some obscurity of diction, and fancy that the average medical student will be at a loss after reading the text of the book to know what are the precise signs by which he might discriminate between ante and post-mortem burns. Thus, it is stated (p. 336) that burns in the living body caused by substances which do not char and destroy the tissues, produce two characteristic appearances—redness and vesication. The redness seen after the infliction of burns during life is next described, and then the blisters, in which it is laid down that the albumen is more abundant when wholly due to vital action than when the burn, being inflicted at the point of death, the vesicle forms when life is extinct. But, it is added, "these appearances belong equally to burns made at the point of death and to those made twenty hours previously." From this it would almost appear

that the authors thought that the appearances exhibited by burns made at the point of death were generally identical with those made twenty hours before death—a conclusion which is at variance with other statements made by the authors.

In the portion of their book devoted to a description of death by lightning, we find a brief summary of most that is of practical importance relative to this most interesting mode of death. Yet we note that there is an entire absence of any mention of the effects of different forms of electric discharge upon the animal body, a subject which has been carefully elucidated by Dr. B. W. Richardson. This section of the volume will also, we venture to surmise, be disappointing to those who are skilled in physical science. Judging by their language, the authors appear to hold most materialistic views as to the existence of one electric fluid. Thus, in describing what is known as death from the *return stroke*, they say—"Death may be caused by an electric shock other than the electric stroke. This happens when a cloud near the earth is negatively electrified, while the earth is positive, and the human body serves as the conductor, by which the equilibrium is restored." In reality death from return stroke is as much a death from lightning stroke as is any other form of death from lightning. A short account of the theory of electric induction would have been much more intelligible than the description we have extracted; which, indeed, conveys no real idea to the mind of the reader as to what a return-stroke is. It would appear also from the text of the book that the authors thought that the mere passage of an electric current through articles of iron and steel was sufficient to communicate to them magnetic properties. The true relation of electrical currents to magnetic properties, and the exhibition of magnetic properties by all metallic conductors and electric currents are omitted.

The interest attaching to the subject of personal identity has, during the last few years, been all-absorbing. The Tichborne trial has attracted an undue amount of public interest, and the proofs and disproofs of the personal identity of living persons have been the subject of conversation in every drawing-room and every tap-room. Hitherto the cases quoted in the text-books of medical jurisprudence were those of historical rather than recent interest; but since the close of the leviathan trial it might be expected that the lesser cases would in those works give place to the greater. Unfortunately, when the second edition of Dr. Taylor's 'Principles and Practice of Medical Jurisprudence' was passing through the press in 1873, the second Tichborne trial was still dragging along its weary length. We know, indeed, that the distinguished author kept back the proof sheets containing his comments on the marks of personal identity on the claimant of the Tichborne estates, hoping against hope that the trial would shortly come to a close; and that it was only on the

advice of the late Chief Justice Bovill that the edition was at length sent to the press minus the sheets already prepared. Dr. Taylor's volumes are hence deprived of much of the interest they would otherwise have possessed. The loss has in some measure been repaired by the publication in the 'Guy's Hospital Reports' of the omitted remarks on tatoo-marks as evidence of personal identity. It is to be feared, however, that they have here much less prospect of coming before the medical and legal professions than they would have if they had appeared, as it was intended, in the body of Dr. Taylor's now classical volumes. Even in the work of Drs. Guy and Ferrier, though bearing date September, 1874, an appendix to the chapter on personal identity has been added, having exclusive reference to the Tichborne case, for the sheets containing the chapter on personal identity were already in the press ere that case was concluded. Dr. Taylor confines his observations to tatoo-marks exclusively, whilst Dr. Guy enters into the whole question of the identity or non-identity of the claimant with the real Roger Tichborne. We must say that neither author gives us entire satisfaction. Dr. Taylor is as usual full and luminous in his description of all that relates to tatooing, but his remarks lack condensation. He appears to us also to be in one respect decidedly prejudiced and unwilling to receive evidence, since he seems to disbelieve the assertion that tatoo-marks may disappear in the course of time. He says that "it has been rather hastily assumed that in a certain percentage, tatoo-marks spontaneously disappear in the course of time." He admits, however, that in a few cases these marks may fade or become less visible; but he adds that "the fading of these marks most probably arises, not from removal of the colouring matter by the absorbents, but from the fact that in some cases the tatooing has been superficially performed on a thick skin, with colours of a fugitive kind or too much diluted." He sums up by saying that the theory of absorption into the neighbouring absorbent glands "to explain the removal of insoluble powders like vermilion or charcoal is unsatisfactory," and that tatoo-marks once properly made are practically indelible. Nevertheless he adduces instances where tatoo-marks were actually detected during the process of at all events partial disappearance, the pigment having been plainly seen in the neighbouring glands as well as in the absorbents leading from the tatoo-marks to those glands. We agree with the doctor that it would in all such cases be more satisfactory to separate the insoluble colouring matter, and not to rely upon colour as an absolute proof of the presence of the pigments. We have ourselves seen in the dissecting room glands and absorbents in the proximity of tatoo-marks so coloured with pigments as to leave no doubt whatever in our mind that the marks had actually undergone to some extent a process of partial removal by absorption.

In his description of the Tichborne case Dr. Guy follows perhaps too implicitly the summing up of the Lord Chief Justice to be quite fair in his remarks, which, indeed, are considerably biassed; but we believe his conclusions to be in the main correct. He introduces, moreover, much matter that is irrelevant to a work of medical jurisprudence, such as the ignorance exhibited by the claimant, his supposed loss of intellectual power, his forgetfulness, and the like: matters of supreme importance to the determination of the question of the identity of Orton with Roger Tichborne, but still matters not strictly pertaining to the purview of the physician; and it is to be regretted that in a short manual the author has not strictly confined himself, to his immediate subject. Notwithstanding these blemishes—or, rather, redundancies—we can conscientiously recommend an attentive perusal of this most interesting appendix to the work to all medical men. The authors, too, are to be commended for the manner in which they have introduced engravings of the portraits of the real and of the would-be Roger into their pages, and the manner in which they have availed themselves of the skilful assistance of Mr. Piercy, the memorial portrait painter. Mr. Piercy's clever mode of so adjusting the portraits as, without impairing their accuracy, to bring out the salient points of difference of form and feature, is very happy.

If the subject of the identity of a living person is beset with great difficulty, what shall be said as to the identity of the dead? There is, in truth, no branch of medical jurisprudence beset with greater difficulty than this. The alterations produced in the features within a short period after death are so marked as to deceive even those most intimately familiar with those features during life; and it is perhaps never safe to speak as to the identity of a corpse without the examination of marks, defects, or deformities as additional means of verification. Even then we may be deceived, as the following case related by Wharton and Stillé from Henke will prove:

"The body of a man between sixty and seventy years of age was found slightly embedded in sand, on the bank of a river; both eyes had been picked out by hooded crows, but decomposition had made no progress. The left ear and the first finger of the left hand were wanting, having the appearance of having been lost in early life. The body was conveyed to a suitable place, and persons were requested by advertisements to come and identify it. After some time two young women claimed it as the body of their father, who, they stated, was a lawyer, that he was in the habit of leaving home for two or three weeks at a time, without informing them where he went, and that he had lost the left ear, and first finger of his left hand. They apparently recognised the clothes and the body, and gave vent to expressions of grief on the event. Subsequently doubts in the mind of one sister were overruled by the confident affirmatives of the other. The funeral took place accordingly, and was attended by the daughters and friends of the supposed

deceased lawyer. Returning from the funeral, the boatman of the ferry which they had to cross asked them for whom they were in mourning, and, upon receiving their answer, laughingly informed them that he had, only half an hour before, ferried their father over alive and well, and directed them where they would find him. This, to their great joy, proved true."

On the whole, we know of no manuals of forensic medicine so excellent as those of Dr. Taylor and Drs. Guy and Ferrier. Dr. Taylor's is unequalled in its elaborate and careful record of cases, but is, perhaps, too voluminous a work for the ordinary student. Dr. Guy's manual, on the other hand, forms a useful handbook, and is of such moderate dimensions that the reading of it is a task not beyond the powers of every student of medicine.

Of Dr. Reese's 'Manual of Toxicology' we cannot speak very favorably. The book is fairly well got up, and forms, doubtless, a useful manual. The author has, however, followed Dr. Taylor almost slavishly, which is, perhaps, a sign of his own not very extended practical knowledge of the subject of which he treats. Dr. Taylor's name appears, indeed, on nearly every page of the volume. A few good American cases are given, which add to the value of the work. Dr. Reese has evidently made himself acquainted with what has been written in English by approved writers on toxicology, and he occasionally ventures to quote such well-known French authors as Orfila and Tardieu. He appears to have unaccountably omitted to refer to most that has been done by the great German toxicologists, with the exception of Casper, who is quoted a few times, but whose writings on toxicology form the least important of his splendid contributions to forensic medicine.

The third edition of the well-known work of Wharton and Stillé maintains the high reputation of its predecessors. The three handsome volumes in which it appears have had the advantage of careful editing. The first of these still appears under the auspices of its original author, Dr. Wharton, and treats exclusively of mental unsoundness and psychological law; and since the speciality of psychological law has, since the publication of the previous edition, in 1860, undergone a revolution, it was necessary to write a substantially new treatise. This the accomplished author has done with marked ability. The volume is filled with the record of instructive and interesting cases, and innumerable authorities are quoted, so that the book is rendered not only a valuable text-book for the medical jurist, but also an indispensable addition to the library of the legal practitioner. We specially commend to the perusal of our brethren the study of the chapter on "moral insanity," in which views are inculcated as far removed from sentimental romanticism as from the repulsive barbarism which at one time disgraced our treatment of

crime and insanity. The key-note to the position maintained is thus stated by Dr. Wharton in his preface:

"Since 1860 a great change has taken place. Before that period, we may say generally, there had been no positive and final repudiation by psychological science of the theory of criminal monomanias. Since then medical as well as psychological science has rallied, and from all quarters there has risen, as will hereafter be shown more fully, almost an unbroken denunciation of a scheme of psychological romanticism, which sober-minded men have learned to feel is as repugnant to science as it is hostile to society. And this advance of science towards a common reconciliatory stand-point is now met [in America and on the Continent] by a corresponding advance of law. It has been just stated that one of the causes of early judicial confusion on this topic was the revulsion from the excessive punishments assigned by the old law to offences of even lighter grade. Civilisation was shocked at seeeing a man who, from nervous, or mental, or physical disorder, was incapable of cool premeditation or exact intent, hurried to the gallows for what might be a comparatively venial crime; and it was to the desire to save such that the toleration of the idea of irresponsibility in such cases is in a large measure traceable. But it was soon found that this enlargement of the idea of irresponsibility worked badly. It exposed many persons virtually sane to the pains and penalties of insanity. It enfranchised a dangerous class of outlaws, too insane to be punished for crime, and yet too sane to be restrained. It involved, on the part of the State, the abdication of one of its chiefest functions—the building up of a right moral sense in those of its subjects in whom such moral sense is deficient."—Preface, xiii.

But to enter upon the wide field of the medical relations of insanity would be to undertake a task to which our space would not enable us to do adequate justice. Indeed, an article itself would have to be devoted to this topic alone. We must with reluctance part from these interesting volumes. Their perusal has afforded us much pleasure. Dr. Reese's work is, perhaps, least satisfactory of them, since we should have desired him to have more explicitly admitted that his manual is a mere compilation of the writings of others rather than an original manual of toxicology.

XI.—The Controversy on Disposal of the Dead.[1]

I.

The long list of books, essays, and papers that lie before us at this moment testifiy to the interest that is now felt on the subject of the disposal of the dead. In the controversy that has been and that is still being waged varied combatants present themselves. The hardy man of science and the studious divine, the enthusiastic layman and the sympathetic laywoman—indeed, we might include in the controversy all who expect to be disposed of after death without greatly perverting the history of our time in relation to the question that lies before us.

The writer of the article in the 'Popular Science Review' assigns, as the reason for so much controversy and so much difference of opinion on a very simple subject, the diversities of sentiment which have ever existed, and which still exist, between different families of men on the solemn subject of death and after-existence. To bury, to embalm, to cremate, he asserts, are acts the inclination to either of which depends altogether upon the disposition of those who have to carry these acts into practice. This disposition, he further assumes, depends, not on reasoning, but on instinct. This instinct depends, not on accident, but on the most veritable of all human endowments—on the organic origin and build of the man, and of the men from whom the man springs; in other words, upon racial and family dispositions and qualities. From this point he traces out the instinctive peculiarities of the representatives of the different civilisations, and comes ultimately to the conclusion that, in this country, a modified system of burial in the earth is the only possible large reform for the interment of the dead that is possible in the present civilisation.

The view that the varied modes of disposal of the dead are indications of varied forms of civilisation is indirectly supported by all the

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[1] 1. *Cremation of the Dead: its History and Bearings upon Public Health.* By William Eassie, C.E. With Illustrations. London, 1875.

2. *Cremation: the Treatment of the Body after Death.* By Dr. Henry Thompson. 2nd Ed. London, 1874.

3. *On the Disposal of the Dead.* By Dr. Richardson, F.R.S. *Popular Science Review for April.* London, 1875.

4. *The Disposal of the Dead. Indian Medical Gazette.* Edited by K. McLeod. Calcutta, April 1st, 1875.

5. *Letters in the 'Times' Newspaper on the Subject of Burial.* By J. Seymour Haden, Esq. 1874-5.

6. *Proceedings of the American Medical Society.* June to December, 1874.

7. *Suggestions to Burial Boards providing and managing Burial Grounds.* By the Inspector under the Burial Acts. 1871.

8. *Burials in Charcoal. Journal of Public Health,* September, 1855.

other writers whose works have reached us. On the whole, the balance of opinion on the two questions—"earth to earth," or "ashes to ashes"—is pretty nearly equal, and the care with which either ceremony is performed shows that both are regarded with befitting solemnity, even by the most uncultivated societies. We know how, amongst our poorest and most ignorant masses, the most respectful and, we may add, costly attention is paid to and for the act of burial. The same care and cost is in like manner paid by the poor of other communities for the act of cremation. On this ground, therefore, of mere personal respect for the dead the one plan is much the same as the other in value.

The writer of the article in the 'Indian Medical Gazette' sums up all the modes of disposal of the dead except two. The Hindoos, he says, burn their dead; the Mahomedans bury without any coffin; the Europeans bury with a coffin; the Parsees expose their dead to be eaten by birds of prey. To these, if he had added that they who live on the sea commit their dead to the deep, and that a few wealthy persons still call in the embalmer for the exercise of his skill, the narrative would have been complete.

We are indebted to this author (who, by the way, though he does not give his name, we suspect to be Dr. K. McLeod) for one or two graphic pictures of the mode of cremation that prevails amongst the Hindoos, and of the mode of burial without any coffin which prevails amongst the Mahomedan populations. As these and other modes of disposal of the dead are each being considered and discussed at the present time, we may with advantage cull one or two interesting details respecting them.

II.

To commence with the Hindoo system—that is to say, with the mode of cremation in its natural home—we read the following facts: —The body is dressed in its best garments, and, after being decorated with flowers, is placed in the sitting posture, and is surrounded with piles of wood, the richer classes using the scented sandal-wood. Then, if the deceased be a noble or native magnate of some parts of India, the heir first breaks open the skull of the corpse with a hatchet (to make sure, we presume, that the assumed dead man shall not be burned alive), after which he (the heir) lights the funeral pyre. If there be sufficient wood, and the wood is a good fuel-wood, and has been artistically arranged, nothing offensive is presented to either the sense of smell or of vision. The devouring element rapidly envelopes the different faggots until the whole is a mass of blaze, giving the beholder a very vivid idea that the "rite" of suttee could not have entailed a very painful or prolonged death. Even should the looker-on allow the wind to pass between the

flames and his nobility, he distinguishes nothing except, perhaps, the odour of burning wood, and he sees nothing except fire. Above the blaze and below the smoke, when the latter occurs from damp wood, there is a bright ethereal glimmer, somewhat resembling, but more diamond-like by far, the appearance of the distant atmosphere on a hot day. Then, as minutes pass, this glimmer subsides, and the flame lessens until nothing remains but an insignificant heap of greyish ashes. The body has changed to something else as different as the soul in its new Pythagorean abode. A pyre as big as that of Sardanapalus affords no protection to the ashes of the dead. Everything has gone, not, perhaps, to dust, but into those still more subtle ultimate elements from which the dust itself is derived. There is, indeed, as a result of a well-conducted funeral pyre, an utter annihilation of the body. It can never, says our author—it can never again, as a substance, do either harm or good, either by resolution or by putrefaction. It has, in short, ceased to exist. Of course consumption is not thus complete at every funeral pile. At most Hindoo burning-places bodies half destroyed by fire may be seen in great numbers, and it not unfrequently happens that during the process of burning there is some unpleasant effluvium. This, however, is simply the result of want of precaution or want of fuel, and is, therefore, only, or at least chiefly, noticed at the funeral rites of the poor and needy. With sufficient wood the destruction may always be rendered as complete as has been described.

From this excellent description of the Hindoo method of cremation we may turn with interest to an account of a similar process, as it is conducted by the subtribe of Path-Utes known as the Cotton Wood, Corn Creek, Spring Mountain, and Pah-rimp Spring Indians. The description is supplied to the 'Transactions of the American Philosophical Society' by Mr. W. J. Hoffman, who in 1871-2 was attached to Lieutenant Wheeler's expedition through the territories of the American Indian tribes. The tract of country in which Mr. Hoffmann found the practice of cremation to exist lies between 110° and 115° 35′ west longitude and latitude north 35° and 36′. Spring Mountain is the stronghold of the tribe, and is located just north of the "Old Spanish Trail." Upon the death of one of these Indians, Hoffman tells us, a pile of wood is prepared in the immediate vicinity; this is so arranged as to form a rectangle, to the height of from two to three feet. The corpse is laid upon this, when the fire is started, after which wood is continually thrown across the pile until the body is reduced as much as possible. Mesquite, pine, and cedar are usually employed, and form excellent coals and an intense heat. All the remaining property—as wearing apparel, arms, blankets, dogs, and horse (if the deceased possessed any such) —is also burned. According to the belief of these Indians, when an Indian dies his spirit goes to the East, which they consider the

"white man's hunting-ground," and where he would be unable to hunt were his spirit deprived of these valuable aids. The remains are then covered with earth. Whether they were really buried the author could not ascertain. Amongst thirty subtribes of the same Indian region the Pah-Utes is the only one that "cremates."

Such is an outline of primitive cremation as it is at present carried on in uncivilised nations. How far it resembles the ancient methods of Greek and Roman days our readers are as capable as ourselves of forming an estimate. The modern civilised cremationist discards these methods of cremation. They are to him rude and costly and slow. He calls into his aid the skill of modern science, and if he does not destroy the dead organism and tear it into its elements with quite the same rapidity as the noble author of the "Coming Race" with its "Kill force," he makes some approach to that last-named wonderful imaginary agency. In the following description we take from Sir Henry Thompson an account of an experiment in which he cremated the body of an animal that weighed no less than 227 lbs., and was not emaciated. The body was placed "in a cylindrical vessel about seven feet long by five or six in diameter. The interior of the vessel was already heated to about 2000° F. The inner surface of the cylinder was smooth, almost polished, and no solid matter but that of the body was introduced into it. The product, therefore, could be nothing more than the ashes of the body. No foreign dust could be introduced, no coal or other solid combustible being near it; nothing but heated hydrocarbon in a gaseous form and heated air. Nothing was visible in the cylinder before using it—a pure, almost white, interior—the lining having acquired a temperature of white heat. In this case the gases, given off from the body so abundantly at first, passed through a highly heated chamber, among thousands of interstices made by intersecting fire-bricks laid throughout the entire chamber lattice fashion, in order to minutely divide and delay the current and expose it to an immense area of heated surface. By this means they were rapidly oxidized, and not a particle of smoke issued by the chimney; no second furnace, therefore, is necessary by this method to consume any noxious matters, since none escape. The process was complete in fifty-five minutes; and the ashes, which weighed about five pounds, were removed with ease."

Mr. Eassie, whose enthusiastic work on cremation we have included in our list, in speaking of the results of the cremation of the human subject as practised in a Siemen's furnace in Dresden, says that in the course of the process nothing whatever is seen that can in any way be considered objectionable. The body remains of itself perfectly motionless and without visible contraction or convulsion. Several late human cremations, he states, have been purposely witnessed by eminently scientific men and others, through the

glass panel door which is always provided for the use of the manufacturing operator, and the utter absence of anything which could prove the least distressing to the mind, the eye, or the imagination is vouched for by all. The current of combined air and gas simply plays upon the body with a transparent flame until the whole becomes incandescent. There is not even the least effluvium. Once incandescent the body soon assumes a hue of translucent white and then speedily crumbles into ashes. Entering still further into detail Mr. Eassie suggests that if the practice should at all become general the body to be cremated would be enclosed before cremation. The body, he thinks, would be placed in a coffin made of some light material, and taken in due time to the mortuary ready for conveyance to the cinerator; and as it is desirable that the ashes of the body should be kept separate from those of any coffin, a shroud of some imperishable material will be carefully sought after by inventors. The ancient Greeks, he adds, made use of asbestos, which is a fibrous form of horneblende; and those Egyptians who performed cremation enclosed the body in a receptacle of amianth, a similar incombustible mineral substance. Whether these materials will resist the intense heat of a Siemen's furnace, he (Mr. Eassie) is not able to say, but wood, at all events, is likely to be rejected on account of the residue of carbon which might not be easily separated from the more precious relics. Lead would be equally objectionable. In all probability the most suitable material for the inner coffin, which alone is to be submitted to the impingement of the hot blast, will be zinc. This metal would entirely disappear in the furnace heat, the reason being that it is volatile, and would distil off below the minimum temperature that would reign in the chamber of the apparatus.

This same author (Mr. Eassie) further prearranges for the changes in the service or ritual in the case of a dead body disposed of by cremation. He tells us that in one Dresden arrangement the body is lowered into a receiver below, and the idea of interment is thus in a manner preserved. In the English arrangement,—which by the way has not yet been carried out on the human subject,—the coffin is made to slide gradually into the receiver like a ship launched into water. The anguish induced by the moment of departure is in this way, he opines, somewhat ameliorated, as there is no noise of lowering machinery to grate upon the ear. He adds suggestively that at certain appointed words in our beautiful funeral service—for instance at the words "ashes to ashes"—a curtain might be partially withdrawn, and the body, enclosed in a suitable shell, would gravitate slowly into the chamber of the apparatus, which would then immediately close noiselessly, to be opened only after the due reduction of the body. The utmost privacy would be ensured, and no strange eyes could gaze upon the body during the period of incineration. Mr. Eassie further suggests that the funeral

service could if desirable be made to occupy the whole of the time necessary for sublimation; or an eulogy or other reference to the departed might form the subject of a discourse. The ashes could afterwards be collected and reverentially placed on an urn and conveyed to their last resting place.

The uncivilised and the civilised methods of cremation, the proposed and the actual, are now before the reader. We may contrast these methods with some others that are suggested as modifications of burial in the earth.

III.

A modification of the ordinary system of burial common in this country is proposed by Mr. Haden as an improvement on the furnace system of cremation that has just been narrated. Mr. Haden proposes practically the mode of burial which is practised by the Mahomedans, with this difference, however, that whereas the Mahomedan places the corpse directly in earth, enclosed at most in a shawl or garment, Mr. Haden recommends that it be enclosed in a wicker basket or coffin surrounded with flowers or enclosed in a layer of charcoal if the cause of death has been an infectious disorder. He instructs us that the body "as soon after death as may be is to be sponged, the eyes are to be closed, the chin supported, the limbs composed, and the hands crossed upon the breast. Superfluous bedclothes, together with the impediments and rejectments of the sick room, are to be removed and a window is to be opened a few inches both at the top and at the bottom. The papers of the deceased may then be examined, and, if these contain nothing to forbid it, the first preparations for the funeral may be made in the following way. As part of the ordinary stock-in-trade of every turner, brushmaker, or basketmaker will be found, nested one within the other, and of every form and dimensions, the necessary covering or coffin; at every herbalist's or florist's, its garniture. Both being light and portable may be delivered at the house in an hour or two, and the body may be at once laid in it and strewn (except the face and hands, which should be left exposed) with its evergreen covering. All this may be done by the nurses or older servants or members of the family, and no stranger need be admitted. There is now ample time to consider arrangements—for the visit of the physician or surgeon charged to verify the fact of death, to telegraph to friends, and to make final preparations for the interment. The morrow come, and everything prepared inside and out, the necessary agents for the interment will enter the house for the first time and the last, and remove the body in a suitable carriage, either by railway or by water, to its resting-place outside the city, one of the male representatives of the family in every case accompanying it. There will be no procession through the streets—no opportunity for display—nothing to elicit either the sympathy or

the criticism of the neighbourhood (both on such an occasion equally out of place); but, arrived at the cemetery, the body will wait in the mortuary chapel attached to it with those who are to be present at its interment. These, having been informed of the death, will go and return, as their desires, affections, or respect for the dead impel them. The assembly will be in the chapel, and at the grave-side only, where the mourners, men and women (for since there is to be no public display both may go), will find the trellissed coffin on its bier—garnished and beautified by loving hands—awaiting them. Not a word of our burial service will be omitted, though more may be said in the chapel and less at the grave-side, and then all will be over. There will be no reunion at the house of death. The conventional feast will not be spread. The formal reading of the will will be at the office of the legal adviser of the family on a day appointed for the purpose; and the inmates of the house of mourning will return to it and be allowed to remain undisturbed. Next day every one will to his business.'"

In the article on the disposal of the dead in the 'Popular Science Review,' to which reference has once before been made, the author holds a similar opinion to that which has been quoted from Mr. Seymour Haden. He insists that "it would be vain to construct the best burial-ground if the present system of enclosing the dead in coffins of wood or iron or lead were to continue. The coffin should be nothing more than an easily destructible shroud, in which the mortal remains may be concealed from view until they are deposited in the earth. The present coffin is after the mode of an Egyptian sarcophagus, and is probably an imitation of that receptacle. In the form of this receptacle there is nothing objectionable, and if the popular taste wills that it shall be maintained, so be it. But the structure must be so modified that the instant the body is placed in the earth it shall either be in direct contact with the surrounding earthy matter or shall be separated from it by some simple organic material that is easily and rapidly destroyed. The newly proposed wicker coffins would probably answer the purpose intended, fairly; but they have the fault of not being sufficiently destructible. A return to the ancient bier and to the primitive mode of simply enveloping the body in cloth would be by far the most rational modification. It is presumed by some who advocate this direct mode of burial that interment should in all cases be carried out within a short interval after death; that a period of not less than thirty-six hours should be allowed to elapse between the cessation of life and the disposal of the lifeless body in the ground. There can be no doubt that the method of placing the body in the coffin, and of partially or temporarily closing it up, has led to much error in the manner of detaining the dead among the living, and it is not less doubtful that when, in any

instance, actual decomposition of the tissues has commenced, the time for interment, however short it may be, has fairly arrived. The system of burying without the coffin would therefore in a sanitary point of view be of advantage. It would lead to interment, in every case, so soon as the direct evidence of decomposition had set in, and in the majority of instances that would be within forty-eight hours from the time of the demise. Third-day burials would become the rule. This period would be sufficient to establish the fact of death on the one hand, and to prevent injury to the living on the other."

In relation to the kind of earth that should be placed in the cemetery, and in which the dead should be buried, this author enters into some minuteness of detail. According to his view, "the construction of the soil of the burial-ground is of first moment, and might readily be made matter of legislation. The soil that is most fitting for this purpose is a fine carboniferous mould, or a mixture of carbon and sand. In such a soil the complete removal of the body might, under proper conditions of burial, be secured within a period of five years; and in such a soil renewal of burial might be carried out after every such interval. In Naples it has been customary to bury in pits of earth in which lime has been placed; to bury so many bodies in one section on a given day, to allow that section to rest for a year, then to remove the whole of the earth of the section with its organic remains, to refill with new earth and bury again for the year in the new earth. In this country such a prompt system would not be tolerated; but the method of burial in a destructive, but more slowly destructive, bed would meet probably every view, without creating undue prejudice at the commencement of the reformation. In some localities a natural soil would yield all that is wanted for a perfect burial in earth. In other localities the earth would have to be specially constructed. A series of carefully conducted experiments on the destructive powers of various earths are required before a perfect system can be evolved. It will probably be found that an earth composed of equal parts of fine carbon soil, sand and lime, would be the most rapid of all combinations for the destruction of the animal matter with absorption of the products of decomposition. In a cemetery correctly constructed, with twelve feet of prepared earth as its basis, the soil might remain undisturbed, except for the purposes of burial, for many years. Long enough, certainly, for the burial-place of the majority of the dead to be forgotten, and for the dead body to pass into entire reunion with the earth from whence it sprang. After a given and due time, without any injury to sentiment, the soil could be removed in sections, and could be resupplied with new matter for new burials.

"The artificial soil which would prove the most effective for the

purposes of burial is suggested from the facts the author has gleaned during direct observation of the action of different substances on dead organic matter. Specimens of such matter buried in pure carbon, in virgin carboniferous earth, in a mixture of carboniferous earth and sand, and in this latter mixture to which lime had been added, were found to undergo quick resolution, in the last most effectively, in the first least. A fresh carboniferous earth answers exceedingly well, far better than simple vegetable carbon. The rapidity with which it deodorizes even decomposing animal matter is most remarkable. It may be said to act in a matter of minutes. The rapidity with which it produces destruction of the organic substance, especially if it be kept dry, is equally surprising. The complete destruction may be included in from twenty to thirty weeks. It is worthy of remark, however, that all the parts of an animal body are not equally destroyed. The integumentary parts and the membranes are more slowly destroyed than the muscular, and the muscular parts are more slowly destroyed than the nervous. The bony parts are more resistant to destruction than the integuments, and the pigments are more resistant than bone.

"It is not assumed that the above-named description of a prepared earth for the cemetery is perfect. It is an approximation to the truth. A carefully conducted series of new experiments are required to bring out the precise facts."

It is possible, and indeed probable, that in course of time many of the details which we have quoted will gradually come into action. The most probable reform is that which relates to coffins. It is not likely that the English people, in imitation of Mahomedan populations, will ever cast the body of the dead man into the ground "like a dog" when it is committed to the earth; but that the heavy wooden coffin and the costly covering of lead will be dispensed with we have little doubt. At this moment that we are writing a collection of wicker coffins, on Mr. Haden's plan, is being exhibited at Sutherland House by permission of the Duke of Sutherland; and although we do not suppose that such exhibition will in itself be more than a nine hours' wonder, it indicates at least by the fact that it is permitted and patronised a tendency on the part of the people to consider, under a new light, an old and hallowed subject.

IV.

The silent garden of the Parsee, in which he exposes his departed to the havoc of the carnivorous birds of prey, is a practice of disposal of the dead which amongst the civilised nations of the earth would not, we think, gain friends even if the birds of prey were ready to perform the task of burial. Diogenes himself, sharp as was his satire when he begged for the little stick to be laid by his

dead body that he might drive off the carnivora, and trite as was his argument founded upon the request, could hardly persuade the modern European, American, or other representatives of civilisation, as it now exists, to follow the Parsee to the place of his dead.

V.

The mode of disposal of the dead by the process of embalming, a mode very ancient, and, in respect to the disposal of illustrious personages, all but universal, is still likely to maintain its place. In America this method has recently undergone a revival, and it is not uncommon in England to hear that an embalmment has been performed. Quite recently in the 'Medical Times and Gazette' a series of lectures on the art of embalming have been published, in which all the necessary details are fully supplied. In the article in the 'Popular Science Review' the facts about embalmment as a modern practice are condensed in a few lines, and the circumstances under which it is justifiable, because useful, are defined.

Embalming, says the author, has in these days been brought to such "perfection that it may, when required, be effected without danger to any one except to the operator. It is performed rapidly by the process of injection of a preserving fluid into the intestines of the dead body. This solution consists of chloride of zinc dissolved in water; and when the process is effectively carried out, the body is left with all its tissues solidified, so that it is all but as rigid as stone itself.

"In the case of persons who die far from their home, and whose friends wish to have them retained unburied until they can inter them in the same burial-ground with other members of the family, the feeling which dictates embalming is fairly gratified. In the case of unknown persons who are found dead, and around whose deaths some doubts hang which nothing but identity can solve, the feeling which dictated the preservation of the body may be sanctioned by the requirements of public justice. But when the desire for embalmment is meant only to gratify a morbid craving, on the part of a few living persons, to retain the mere animal remains of the dead, then the conditions are changed, and the only circumstances that can justify the demand fully are those which relate to history. It may be urged that great historical personages may be embalmed with advantage; it may be urged that persons who are not in any sense great, but who from some peculiarity of physical construction are of interest to the natural historian, may be embalmed with advantage; and here we think the argument in favour of embalmment rightfully ends." Whether such historical personages, illustrious or peculiar, would like to be subjected to the process if they knew the base uses to which they might ultimately come, we will not decide.

VI.

The suggested plan of burial in the deep sea carries with it many inducements. It ensures a method for the immediate disposal of the dead as effective as cremation, in so far as the living are concerned, and, from what we know of it as a practice at sea, it is, to the mourners, as simple and inoffensive as the burial in the earth. It has the disadvantage that it is not readily available to inland populations, and that it too effectively disposes of the body to satisfy, in every case, the demands of the law.

VII.

Practically, we are at this moment driven to deal with cremation and burial in the earth as the two modes for the disposal of the dead most consonant with our modern civilisation. For our part, we are free of all personal prejudice as to these modes; or, if our own feelings were really influenced, it would be towards cremation. We confess that we feel as the English lady, whose remains were subjected to the cinerator at Dresden during last year, that the readiest, most certain, most wholesome way of being disposed of after death is to go through the fire and remingle with the air. What therefore we have to say in respect to these methods and their relative values will be based on what seems to us the true scientific and legal aspects of the question.

Speaking, in this sense alone, of cremation, while we admit for it that it is quite practicable as a general process, inexpensive, free from any except morbid objections in matter of feeling, and as a mere sanitary measure excellent, there are against it two objections which seem to us so powerful as to render it impossible of introduction, for general application.

The first of the grand objections to cremation has been advanced with great force and effect by Professor Mohr. It is that if the process were universally adopted and all animal remains passed for ultimate destruction as organic matter into the hands of the cremator, the organic world would lose one of its important constituents. If cremation is to be adopted there must, he urges—and indeed all admit this argument—there must be such complete combustion that not a trace of organic vapour or gas can escape from the retort. Any trace of odours and fœtid compounds escaping from the retort, anything in the nature of a distillation, would be so revolting that no one would tolerate the procedure, and no idea of value from the products thus given off would compensate for the effects that would be experienced. To produce the perfect combustion the temperature must be raised to a degree that shall render the combustion final. The very gases that are evolved from the burn-

ing of the body must pass through heated earthenware lattice work, so that they may be transformed into what can fairly be designated true inorganic parts. Nothing must remain but carbonic acid, nitrogen, water, and ash.

In this complete combustion there is the widest possible departure from the mode in which nature, by her own processes, disposes of dead animal matter. Her plan is to produce in the course of decay secondary or intermediate products which shall again prove of service to the vegetable and, through the vegetable, to the animal kingdom. One of these products, the staple of the vegetable kingdom, is that combination of hydrogen and nitrogen known as ammonia. As yet we have in chemistry no artistic synthesis of this body by which it can be made available for the purposes of organic growth, and so we depend for the natural development of it, we may say exclusively, upon the natural decomposition of dead animal organic remains. What, therefore, would be the ultimate fate of the living organic world if the whole of the dead organic matter derived from the bodies of the dead were swept at once by intense heat into inorganic compounds which have no sustaining property for the vegetable kingdom? In the simple natural process the intermediate product, the ammonia resulting from decomposition, escapes into the air or is washed into the soil, is absorbed by the plants, is transformed into nitrogenous tissue by them and becomes in turn available as food for animals. By this wise and provident means a continuous circulation of ammonia between the animal and the vegetable kingdoms is sustained. If we stop all source of the supplies thus afforded we disturb the balance of natural supplies, we draw upon the ammonia capital of the globe, and in the course of time, if the spendthrift proceeding were kept up, the loss that would be sustained could not but be felt through the whole of the animal kingdom, and could not but lead to a reduction of animal life. In the case of the loss of ammonia the mischief is inconceivable. There is here no order of compensation going on in nature, as in the process of the renewal of atmospheric oxygen by breathing animals, but the deduction is from a finite quantity, and the descendants of the present races would have to bear, should we subject them to the ordeal by our short-sighted policy, the evil of our error as we have now to endure the results of the folly of our ancestors who ruthlessly cut down vast forests, thereby incurring the penalties of drought in some regions and of flood and inundation in others.

What is more, in the act and art of cremation we incur a second evil of deduction from a finite quantity. To cremate we must employ fire: to employ fire is to deduct so much from the finite materials which give forth heat. In a word, we burn the candle at both ends and have no manner of compensation beyond the present satis-

faction rendered to a few minds from the favour or caprice of a fashion that defies natural capital and natural law.

There, however, is another side to this question of economy of the resources of nature which should in fairness be put forward. Sir Henry Thompson sees in the application of the bone ash of the burned dead what we may call a "quid pro quo" for loss entailed by other means. We will, that we may miss no point of the argument, give this author's view in full. Let us, says he, glance at the economic view of the subject of cremation.

"It is not so important as, unconsidered, it may appear. For it is an economic subject whether we will it or not. No doubt a sentiment repugnant to any such view must arise in many minds, a sentiment altogether to be held in respect and sympathy. Be it so, the question remains strictly a question of prime necessity in the economic system of a crowded country. Nature will have it so, whether we like it or not. She destines the material elements of my body to enter the vegetable world on purpose to supply another animal organism which takes my place. She wants me, and I *must* go. There is no help for it. When shall I follow—with quick obedience, or unwillingly, truant-like, traitor-like, to her and her grand design? Her capital is intended to bear good interest and to yield quick return: all her ways prove it—"increase and multiply" is her first and constant law. Shall her riches be hid in earth to corrupt and bear no present fruit; or be utilised, without loss of time, value, and interest, for the benefit of starving survivors? Nature hides no talent in a napkin; we, her unprofitable servants only, thwart her ways and delay the consummation of her will.

"Is a practical illustration required? Nothing is easier. London was computed, by the census of 1871, to contain 3,254,260 persons, of whom 80,430 died within the year. I have come to the conclusion, after a very carefully made estimate, that the amount of ashes and bone earth, such as is derived by perfect combustion, belonging to and buried with those persons, is by weight about 206,820 lbs. The pecuniary value of this highly concentrated form of animal solids is very considerable. For this bone-earth may be regarded as equivalent to at least six or seven times its weight of dried but unburned bones, as they ordinarily exist in commerce. The amount of other solid matters resolvable by burning into the gaseous food of plants, but rendered unavailable by burial for, say, fifty or a hundred years or more, is about 5,584,000 lbs., the value of which is quite incalculable, but it is certainly enormous as compared with the preceding.

"This is for the population of the metropolis only: that of the United Kingdom for the same year amounted to 31,483,700 persons, or nearly ten times the population of London. Taking into consideration a somewhat lower death-rate for the imperial average, it will at all events be quite within the limit of truthful statement to multiply the above quantities by nine in order to obtain the amount of valuable economic material annually diverted in the United

Kingdom for a long term of years from its ultimate destiny by our present method of interment.

"The necessary complement of this ceaseless waste of commodity most precious to organic life, and which must be replaced, or the population could not exist, is the purchase by this country of that same material from other countries less populous than our own, and which can, therefore, at present spare it. This we do to the amount of much more than half a million pounds sterling per annum.

"Few persons, I believe, have any notion that these importations of foreign bones are rendered absolutely necessary by the hoarding of our own some six feet below the surface. The former we acquire at a large cost for the original purchase and for freight. The latter we place, not in the upper soil where they would be utilised, but in the lower soil, where they are not merely useless, but where they often mingle with and pollute the streams which furnish our tables. And in order to effect this absurd, if not wicked result we incur a lavish expenditure! I refer, of course, to the enormous sums which are wasted in effecting burial according to our present custom, a part of the question which can by no means be passed over. For the funeral rites of the 80,000 in London last year, let a mean cost of ten pounds per head be accepted as an estimate, which certainly does not err on the side of excess. Eight hundred thousand pounds must therefore, be reckoned as absolute loss to the costs already incurred, in the maintenance of the system. Thus we pay every way and doubly for our folly."

To this the same author adds in a subsequent paragraph:

"The problem to be worked is—Given a dead body, to resolve it into carbonic acid, water, and ammonia, and the mineral elements, rapidly, safely, and not unpleasantly."

With this proposition we entirely concur. It is in truth *the* problem. Our argument is that the problem is not solved by the process of cremation.

It is perfectly clear that the error which the advocates of cremation fall into when they advance the economical side of the question is twofold at least. They obviously overlook, in the first place, the all-important fact that by the very perfection of their art they break up the most valuable of the products of natural decomposition, viz. the ammonia. If they did not succeed so far their process would give rise to the emanation and distribution of vapours that would be detected by the smell, and the success of perfect purity to every sense which they claim to have attained would be utterly forfeited. On this ground, consequently, the economy they assert is not maintainable in the controversy, The process is one of the direst waste, for by it the very organic matter of the skeleton, which is so valuable as an ammonia-yielding product, is resolved into its primary parts, its nitrogenous plant-forming value unredeemed.

In the second place an error not less important, and exceedingly

obvious, creeps into the advocacy, when the subject of the value of the bone earth comes before us. No one would deny the value of the bone earth as a fertilising substance, and no one would deny, we presume, that in cremation the inorganic bone-dust is very effectively preserved. But it is not easy to see how this precious material—precious as a relic of the dead — precious as manure—can act both kinds of preciousness at one and the same time. We mean by this that if it be stowed away in an urn it cannot fertilise the earth. But we allow that in due course of time it will by chance of fate come at last to the earth, and therefore will not be lost. Here the cremationists are right; but they are wrong, and in this their second great economical error lies, that they consider themselves performing a novel and wonderfully saving plan by burning instead of burying dead bones. As if bones did not resolve in the earth! As if the phosphorus and lime which make up the inorganic residue of bone-matter could ever be lost by the act of banking it in the place from whence it came! We need push the argument no further. The man that burns a bone and puts the ash in an urn is certainly not doing more for the earth than the man who directly restores the bone to the earth. The difference simply is, that the first is a miser, the second a prudent husbandman.

Looking at the subject of cremation, then, from these points of view—looking at it as a conservation or a destruction of the natural order of events and material changes—we hold it to be a failure on the principle of natural economy.

The next grand objection to cremation is of a different order, and although in course of time it might lose its force, it is at present potent in the first degree. The objection is medico-legal. It is urged that if a body destroyed intentionally by poison, or by any other secret and unlawful means, could be carried straightway to the furnace, and, in the course of an hour, could be resolved into its all but elementary form, the course of justice would constantly be arrested; and, not to mince the matter, the cremator would not unfrequently be the unwitting slave and instrument of the murderer.

The difficulty in the way of cremation, from the risk above stated, is not ignored by the cremationists. They treat it as "an episode" in the discussion—a mode of treatment ingeniously expressed, but indifferently reasoned. Sir Henry Thompson would meet this difficulty by having appointed over the dead a new officer—a sort of universal, omnipresent coroner, who should take the place of the "Médecin Vérificateur" of Paris, without whose permission no burial whatever can take place. In cases where there existed any doubt as to the cause of death he (Sir Henry) would take a strong precautionary measure. He would have the stomach and a portion of one of the viscera preserved, say for fifteen or twenty years, or thereabouts, so that in the event of any suspicion subsequently occurring greater

facility for examination would exist than by the present method of exhumation."

We need not dwell at any length on these propositions. In this country, where the coroner has even now so much to contend with in conducting his comparatively simple inquiries—simple, we mean, in so far as the liberty of the subject is concerned—the "Médecin Vérificateur" would indeed stand a poor chance of being able to carry out his onerous duty. Here, in fact, such an officer and such an office would be utterly impossible, and simply repugnant to the genius of the people. The second proposition, that in suspected cases of poisoning portions of the dead body should be kept *in terrorem* over the heads of persons, known or unknown, personally guilty of murder, were even more inconceivable than the first. Where would such relics be stowed? How would the continuance of their identity be maintained? How could it be proved that in the course of "fifteen or twenty years, or thereabouts," between the times when they were stowed away and the times when they were taken out to be analysed, they had been free of all possible chance of contamination? More than this, when a body has to be exhumed there is proof that the person or persons suspected of foul play has or have succeeded in getting the body buried before the suspicion occurred. If such cunning and wicked persons could succeed in committing the body to the earth, could they not, under the proposed new system, succeed also in committing it to the fire, and, if they did succeed, of what value were the analytical skill that has so certainly, under the present system, led to the detection of crime? We speak with no diffidence, because it is our duty to speak with earnestness, and our privilege to speak with the authority of forensic knowledge, on this subject; and we affirm that in the present social state the introduction of cremation as a general system of disposal of the dead would be a direct and effective incentive to the perpetration of the most hideous of crimes. Let no man deceive himself or others on an issue so solemn. The living world is not yet sufficiently purified in its soul to purify itself from its dead tenements of soul, by the unanswerable fire.

VIII.

So we come at last back to the old, old story. The earth gave up to us from itself the substance of our corporeal organization—the earth demands the substance back again. This is the order of nature, and to fight against the fact is waste both of word and labour. On the line with nature we may improve as we like (to invent is impossible), and remain secure. Off the line we may make noise, and astonish the mere lookers-on, but go straight away we cannot. It is open to us to improve the process of burial, and many of the plans which have recently been suggested are deserving of our best consideration. The

suggestions to burial boards issued by the inspector under the Burial Acts indicate on every page the desire to introduce every available improvement, and we do not doubt but that the present agitation will tend to direct public opinion in the right way to some greater advancements in the art of burial. To these advancements we have to refer in detail.

We have already seen what one of our authors considers would be the best soil in which to place the dead. To understand the practical value of his suggestions, and the reforms that would have to take place before they can be carried out, it is necessary to recall the views which are entertained by our Government authorities who hold office under the Burial Acts. The report of the very able Inspector under the Burial Acts (Mr. Holland) comes before us with great effect at this point of our subject. As many of our readers may not have had an opportunity of studying the official suggestions that have been supplied, we shall be rendering a service by devoting a few paragraphs to them. In speaking of the character of the soils of burying grounds as a subject of great importance, the Inspector says that "dry, open soils which readily admit air and moisture, allowing the rain which falls upon the surface to enter readily, carrying air down with it, facilitate decay, and permit graves to be sooner reopened for subsequent interments. Porous soils, mixed with vegetable mould, absorb and decompose the products of decay, and prevent the escape of injurious emanations, if the quantity of animal matter be not too large in proportion to the area, and if the soil near the coffin be left undisturbed until decomposition is completed. Dense clay soils are in all respects undesirable; they exclude air and moisture, retard decomposition, and render it improper to reopen a grave, to nearly its original depth, within any reasonable period. In some such soils coffins have remained undecayed for thirty years or more, and therefore graves in such soils can be used a very limited nnmber of times only. They are, moreover, expensive to drain; they retain the gases of decomposition, and sometimes crack, possibly allowing dangerous exhalations to escape. It is so difficult and expensive to remedy these defects, that it is better to select an open than a clay soil, even though the site be moderately more distant, or more costly. In some such cases the plan has been adopted of enclosing separately every coffin buried in concrete or cemented stone or brick-work, the extra cost of which is to be set against the diminished cost of less excavation, and less rapid filling of space.

"Soils which have no proper mould, and which consist chiefly of stone, may allow of the passage of undecomposed emanations, and it is difficult and expensive to supply the mould in which they are deficient. It is always desirable, before deciding upon a site, to

have the soil examined in various places to the depth of at least eight feet."

Much attention has been paid to the planting of burial places, and we know that now in the cemeteries near to our large towns considerable taste is shown both in fencing, planting, and cultivating vegetation. In his supplementary report on the practice of interment in towns, "Mr. Loudon recommends for planting in cemeteries trees chiefly of the fastigiate growing kinds, which neither cover a large space with their branches nor give too much shade when the sun shines, and which admit light and air to neutralize any mephitic effluvia. Of these there are the oriental arbor vitæ, the evergreen cypress, the Swedish and Irish juniper, &c. For the same reason, trees of the narrow conical forms, such as the red cedar, and various pines and firs, are desirable. In advantageously situated cemeteries, some of the larger trees, such as the cedar of Lebanon, the oriental plane, the purple beech, the dark yew, and the flowering ash, sycamores, mountain ash, hollies, thorns, and some species of oaks, such as the evergreen oak, the Italian oak, with flowering trees and shrubs, would find places in due proportion." The article in the 'Popular Science Review' suggests that under a perfect system of burial there would be demanded a modification of the present plan of planting the surface of the cemetery. The recommendations offered are not very different from those of Mr. Loudon, but they may be noted. The author is of opinion that to surround the place with trees, not too thickly set, to plant small and handsome trees in different parts of the grounds by the side walks and in odd spots where the earth remains undisturbed, would be unobjectionable. The rugged elm and yew tree's shade might still encircle the homes of the dead, but inasmuch as the earth in which the burials are made should be a movable earth it would be impossible, except in particular instances, to plant over any one body any special or lasting tree or shrub. The ground levelled at once after burial should be covered with rapidly growing vegetation; such quick-growing grasses as can be mown and utilised either as food for herbivorous animals or as manure for other land. Thus the products of decomposition from the dead, which by diffusion would find their way to the surface, would be removed, by their transformation into new forms of matter, as rapidly as they were evolved and distributed.

Mr. Holland, who also favours the planting of evergreen and quick-growing trees in cemeteries, supplies useful directions on the position the trees and shrubs should hold in the grounds. It is of importance, he tells us, "that trees and shrubs should not cover too large a portion of the burial ground, and that they should not be too closely planted, or should be thinned out as they grow large, to avoid interfering with the ventilation and with the free passage of air. A suitable disposal of trees or shrubs along roads or pathways

would afford shelter to persons visiting graves, and to funeral processions. It is customary in well-regulated cemeteries to avoid burying close to the boundary fence. On many accounts this is advisable, as there is no law to prevent houses being built and wells sunk close to a burial ground; there is, moreover, a disposition to erect houses in the vicinity of ornamental cemeteries. Much of the evil may be prevented by draining the ground in such a manner as to prevent the water passing into the subsoil of the neighbourhood, and by the adoption of proper regulations as to burial; but it would nevertheless be advisable to leave a belt of land for planting between the fence and the nearest graves. This strip of ground would not be wasted, for part of it might be used as a walk, and part for ornamental shrubs. A surrounding belt of shrubbery would contribute to the seclusion of the ground, and need not be so close as to check too much the free passage of air."

It happens in some localities that these requirements for particular soils and vegetations are next to impossible. The carboniferous soil does not exist near to the communities for which it is required, and in the hard clayey bed of earth which is alone obtainable the resolution of the dead body is long delayed. Cemeteries fixed in such grounds are of all the most difficult to keep in proper condition, and many schemes in the way of draining have been proposed in vain. If any cemeteries are actually dangerous to the community these are, and so long as the cemetery system lasts they will remain an equal trouble to the sanitarian and the engineer.

The best mode of burying in such localities is probably that which was indicated twenty years ago in the 'Journal of Public Health and Sanitary Review.' This consists in enveloping the dead in a layer of charcoal, a process which, says the writer, is nothing more nor less than a slow combustion. As a proof of the effects of charcoal in this respect it is recorded that the Messrs. Turnbull, the well-known manufacturers of charcoal in Glasgow, made the experiment of burying a horse. They surrounded the carcase of the animal with eight inches of charcoal, and discovered that every portion of the carcase, except the skeleton, was destroyed within twelve months. They observed that during the whole of this time there was no trace of deleterious exhalation from the decomposing body; and they further observed that moisture from rain did not materially modify the destructive process. In commenting on this experiment he author adds that he had confirmed it on a smaller scale, and that the had the authority of Dr. Stenhouse for stating that peat charcoal would answer for the same purpose, and that the reduction of the substance to a state of fine division is not necessary.

Since the above-named proposition was made in 1855 the system advocated has many times been carried out, and we believe with success. The difficulties of its extension have arisen from the cir-

cumstances that the introduction of peat charcoal into common use has been much less easy than was originally anticipated, and that the price of ordinary vegetable charcoal has considerably increased. These difficulties, we fear, are not likely to be lessened in the future.

IX.

As we glance over that which we have written and take a final survey of the books which have served us for our texts, we are brought sharply to the conclusion that after all there is nothing before us that is new; nothing on which we are able to form any conclusion of a novel character; nothing that strikingly indicates a great revolution in modes of disposal of the dead. Cremation, with all the fascinations of science which surround it, and with all the advantages which spring from science practically supporting it, is, as we have seen, simply impossible at this imperfect stage of civilisation. It may one day take a prominent place, and it might, under an exceedingly wise legislative enactment, be *permitted* in this country at a comparatively early day. Concerning it we can say nothing further.

On the matter of burials in the earth we are convinced that steady improvements will continue. No one who recalls the old and loathsome grave-yards, and who contrasts these with the new and beautiful cemeteries which chasten and even adorn our towns and villages, can doubt that since the days of Mr. Walker's magnificent protest advancements have been made which that ardent reformer could hardly have anticipated. What, therefore, may be the greater advancements in coming years, when the public mind is so earnestly being directed to and led into a study which it has previously avoided rather than courted, it is impossible to say. But the duty of the man of science, in his capacity as a practical teacher of the people, is clear. It is his duty, without too severely wounding deeply-rooted prejudices, which after all have their origin in the sentiments of reverence and affection, to teach that nature demands for the perpetuation of the living present, not the destruction, but the return of the materials of the dead. That the world of life, constructed from a limited supply of matter, rebuilds itself out of the quarry of death, and that every plan which has for its object the restoration of the body to the earth, with the least possible interruption to the ordination of nature, should be accepted as the wisest plan, the best for the present, the best also for the future generations of mankind.

XI.—Hospital Reports.[1]

Regarding hospitals, and particularly those of the metropolis, with their attached schools, their staff of medical officers enjoying European reputation, their large fields of experience and unequalled opportunities of turning that experience to account for the advancement of medical and surgical knowledge both theoretically and practically, as the chief centres of medical instruction and observation in the country, no division of medical literature would seem to have a higher claim upon our attention than the reports of the work carried on within their walls. As we especially look for the enlargement of chemical knowledge to systematic and continuous work in an extensive and well-ordered laboratory under the direction of one or more chemists of repute, so we might naturally look pre-eminently to hospitals, viewed as laboratories for examining and analysing morbid conditions, and for the extension of therapeutical science. And doubtless medicine and surgery are indebted very largely for the advancement they have made, both directly and indirectly, to hospitals as fields of practice and inquiry. Yet this good result has not for the most part appeared as the direct outcome of our hospitals, but has been filtered to us through the written works and lectures of their physicians and surgeons; and, what is more, has been represented only in one or more phases dictated by the special views of the particular writers or teachers. Consequently there seems reason for the production of reports of the general work done in hospitals; that the extent, nature, and tendency of that work may be known; that the purposes aimed at and the results achieved may be understood, and that the defects and weaknesses in our principles and practice may be displayed by the experience the wards supply. By thus laying open before the profession the doings of the several principal hospitals of London and other chief towns, the professional mind might be made cognizant of the direction of modern medical opinion and of the modes of practice most approved. And as the general public resort to a chemical laboratory to obtain an analysis, so the members of the profession should be able to look to the records of hospital work in elucidation of difficult problems, or might submit questions in pathology and practice to one or other hospital for solution. This subject of hospital utilization is one for an essay, and not for the introductory observations to a notice of Hospital Reports. We

[1] 1. *Guy's Hospital Reports.* Vol. xx. Third Series. 1875.
2. *St. Thomas's Hospital Reports.* New Series. Vol. v. 1874.
3. *St. Bartholomew's Hospital Reports.* Vol. x. 1874.
4. *St. George's Hospital Reports.* Vol. vii. 1875.

may remark, however, that no hospital we know of at the present time approaches a perfect machine for evolving medical truths and fitly advancing medical science. Nor do any of the Hospital Reports published realise to us the actual character of work accomplished within the hospitals. Only imperfectly do they deal with definite questions in pathology and practice submitted to investigation, and very rarely do they set before us comparative results.

As matters stand, the several Hospital Reports published are collections of essays and reports of cases by the physicians and surgeons of each hospital, and by gentlemen—outsiders, in practice here or there—who, as a rule, for the most part have been students of the hospital; and respecting the contained papers, it may be observed that those from outsiders are, voluntary offerings; and, as the maxim directs us "not to look a gift horse in the mouth," such contributions cannot be subjected to a very critical ordeal before insertion (and the less so as the editors of the reports are called upon to make up a certain quantity of matter); they are thankfully received, and prove of varying importance and interest. Again, as to contributions supplied by the hospital staff, they are of a heterogeneous character and now and then look like chance papers, the stray products of the portfolio that have not found their way into the volumes of transactions of one or other learned society; and the impression conveyed by such papers is that they have been thrown out to appease the hungry demand of the editor for "copy." It may be assumed that the editors of Hospital Reports endeavour, for the credit of their volumes and of their schools, to secure the co-operation of their most eminent colleagues; but here again a defect is observable, that those distinguished individuals are not uncommonly limited, and not constant contributors to the reports. In other words, the principal individuals in the hospital engaged in its chief work, the best versed in the operations and experiments performed, and the best trained for research and the deduction of truths, are the least concerned in furnishing records of observations and facts—in a word, in giving the history of its work. Hence it happens that the larger proportion of the contents of the Hospital Report is the production of the junior members of the staff. We would not imply that the work of the latter is not of great merit, nor even that, as a matter of course, it is inferior in quality to what might be produced by their seniors in hospital standing; we are arguing only on the broad general principle that those who have enjoyed a longer and wider range of observation and experience are to be accounted as more able to instruct their fellow-men.

The section of the Hospital Reports under notice that best represents a record of work done is that of statistics. But to these it may be objected that they are deficient in bringing out facts as to pathology and treatment available to the practitioner, and, indeed, also to writers

on any special subject. In the case of the Reports of Guy's and Bartholomew's Hospitals the statistics are very bald, and afford only the dry light that can be used by statisticians. But both the St. George's and the St. Thomas's Reports supply, not only figures, but notes of cases, digests, statements regarding morbid appearances and treatment. But in one and all of them we lament the scarcity of summaries calculated to instruct in pathology and in therapeutics, and of systematic inquiry pursued with the purpose of clearing up the many problems inherent in the principles and practice of medicine and surgery, such as might naturally be looked for as the products of hospital experience.

We are smitten with compunction at what we have written. We have reversed the example of the son of Beor; for we sat down to bless altogether the volumes before us, but some critical spirit has possessed us thus far and perverted our generous intentions. We change our point of view, and now look upon those volumes as collections of valuable contributions calculated to benefit the professional world. We recognise the painstaking work of many of those contributions, and our regret is that we cannot present an analysis of them for the advantage of our readers. Their subjects are very diverse, ranging over the whole area of medical and surgical knowledge, and possess, therefore, a varying degree of interest to individuals according to the particular direction of their studies. The number of separate articles in the four volumes referred to in connection with this notice is very considerable, and to make only a few notes on each of them would involve the production of a lengthened review such as could not find a place for in the present journal. To select some, even without making comparisons between the papers in the several Hospital Reports, would be an invidious task, and one open to much criticism, inasmuch as our private judgment of the interest and value of this or that article must be much influenced by our own predilections and special studies. We therefore defer making quotations, and feel less hesitation in so doing than we might otherwise do, for this further reason, that those papers best calculated to place on a surer footing or to widen our range of professional knowledge are just those which will obtain their due recognition in the pages of this review, in one or more of those analytical articles we especially cultivate, intended to exhibit our present state of information with respect to the principal subjects in medical and surgical science that arrest attention at the present day. There is yet another reason for any apparent neglect of the contents of these volumes, namely, that some of the more important papers are struck off by the desire of their authors and published as separate pamphlets, whilst others appear in a somewhat different dress among the recorded transactions of the medical societies, or are sooner or later expanded into special treatises, or otherwise get absorbed in

the published teachings of their writers. In other words, in the struggle for existence, and barring those to which parental fondness and determination may give a passing existence by spasmodic pains and expenditure, those essays best fitted to maintain an independent and worthy vitality are sure to survive and to become sooner or later objects of attentive consideration and due esteem.

We are glad to observe the amount of support accorded to these volumes in the way of subscriptions, for the most part contributed by former students of the hospitals they respectively relate to. They are well worthy of this support. It might be given with advantage both to editors and readers in a much higher proportion; for these publications are replete with valuable and useful knowledge, and are calculated to keep their readers *au courant* to the latest growth of medical science, and there is nothing like encouragement to improve yet further their quality.

The critical spirit returns to us and compels us, in spite of our admiration of so many excellencies in these volumes, to reiterate our primary observations and expressed views as to what Hospital Reports should be, and to put forward the desire that the so-called Hospital Reports should no longer be a congeries of contributions by a multitude of writers both connected and unconnected with the hospitals, and so far resemble a volume of transactions of a learned society, but be veritable transcripts of the results, theoretical and practical, arrived at in the wards, in the laboratories connected with them, and in the dead-house. That, moreover, they should present series of observations continued over a lengthened period, and directed towards well and previously defined purposes. Clinical lectures delivered to students are out of place in their pages; and scrappy notes of cases, few in number and unaccompanied by a comprehensive review of the morbid states they exemplify, are no better than encumbrances of their space.

XII.—The Leprous Diseases of the Eye.[1]

THE present work gives for the first time a full account of the different affections of the eye that are found to occur in consequence of leprosy. Revision by an Englishman would have considerably improved the style, though we must at once add that the authors' language is easily understood and the matter excellent, giving a clear insight into the nature of these manifold pathological states. A short analysis will suffice to call attention to this book, which we are glad to recommend to all interested in the subject. The authors

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[1] *The Leprous Diseases of the Eye.* With 6 Coloured Plates. By Dr, O. B. BULL and Dr. S. B. HANSEN. Christiania and London, 1873. 8vo, pp. 27.

distinguish two forms of leprosy, the tubercular and the smooth: both are ordinarily well characterised by the difference in their course. Leprous affections of the nerves occur in both forms.

The diseases of the eye may be distributed into those which are the immediate result of leprosy and those which are only indirectly produced by it, as paralysis of the orbicularis following upon leprous disease of the facial nerve.

Cornea.—A superficial opacity commences at the margin of the cornea, and gradually spreads over a greater or less extent of it in most leprous patients; those suffering from the tubercular form are especially liable to this change. It usually occurs in both eyes at the same time, although more marked in the one than in the other, and begins at the upper and outer edge. Delicate vessels can be seen very often with a lens, and sometimes by the unaided eye, to pass from the conjunctival and subconjunctival vessels into the opaque part, and they are sometimes so abundant as to give it a dirty reddish colour. Small, more opaque, white spots can also be seen in and near the margin of the opacity. They are at a greater distance from one another towards the centre of the cornea, and may be traced by focal illumination far within the more uniform peripheral obscuration, separated from it and from each other by perfectly pellucid spaces.

Such nebulæ may appear at very early stages of leprosy; their progress is usually very slow. The periodicity, which characterises the whole disease, may be also noticed in their development.

"The state remains for years unchanged; then suddenly an hyperæmia of the cornea supervenes, during which the whole obscuration may be overcast with vessels extending far over the cornea. When the hyperæmia has existed for some time it goes back again, but leaves a greater obscuration. The more frequently the vascular injection occurs the less completely it usually disappears; and in a more advanced stage we have always found the obscuration also microscopically covered with vessels. As the process advances, more and more of the cornea becomes obscured; but it is an exceptional case when the whole cornea is darkened in this manner. The centre usually remains free; but we have seen cases in which the whole surface of the cornea had become opaque, and had the appearance of dull ground glass."

Small superficial slices of the affected portion of cornea, placed in a half per cent. solution of chloride of soda, showed under the normal epithelium leprous elements of the size of a white blood-globule and even less. Alterations of the corneal cells, which perfectly correspond to those in traumatic keratitis in rabbits, were found in a cornea removed five hours after death. The vascular part of the opacity appears to be anatomically just like the pannus, which can be produced in young rabbits by a thread passed through the eye in

the region of the ciliary body. The author's conclusion is, that the new cells (leprous elements) are derived both from the corneal corpuscles and from the blood-vessels; the greater part indeed from the latter, "because the mass of altered corneal cells does not stand in any proportion whatever to the mass of new cells."

There can rarely be any question of treatment. A strip of conjunctiva round the cornea ought, perhaps, to be excised in a few cases, where the opacity threatens to extend in front of the pupil.

Tuber corneæ.—Instead of being diffused over a large surface in a thin layer, the leprous products may be accumulated into a mass, so as to form a tuber. This is always preceded by a superficial opacity, and commences at the margin of the cornea, in the great majority of cases towards the outer side. Both eyes are usually attacked and the tubers are placed symmetrically on corresponding spots. The first indication of this process is given by a circumscribed conjunctival and subconjunctival injection, of a triangular form, with its base close to the edge of the cornea. This is followed after a time by a yellowish-red elevation, which, as it increases in size, extends on to the cornea; the swelling is not uniform, it slopes gradually outwards, but ends abruptly on its inner side. Its surface becomes uneven. It may at last entirely cover the cornea and prevent closure of the eyelids. It often remains for years unchanged; finally it is absorbed, or in rarer cases ulcerates away, the globe usually shrinking considerably at the same time.

When the tuber is developed in the deep layers of the cornea, it may advance between the lamellæ like a wedge; its colour is then less red, but usually more grey than when superficial. Such growths often appear simultaneously in the same eye, both on the surface and in the deeper layers of the cornea. The more superficial take their origin from an episcleral formation; from this they advance under the corneal epithelium, or pass along the vessels into the middle of the membrane, leaving both the anterior and posterior portions pellucid. The conjunctiva is freely movable over the episcleral growth. The most deeply placed tubers proceed from the region of Schlemm's canal, and extend into the cornea immediately in front of the membrane of Descemet, the spaces between the fibres of the pectinate ligament becoming closely packed with round corpuscles.

The treatment is unsatisfactory. The authors state that—

"The development of tubers on or in the cornea is far more dangerous for the sight than the superficial corneal obscuration; and these tubers are (with the tubers on the iris) perhaps the affections which blind the greatest percentage of leprous patients. In any case very little can usually be done to prevent their growth, which is destructive to the eye.

"Where as yet only episcleral infiltration has existed, where the tuber is just begining to encroach on the cornea, it has occasionally

been possible to prevent further progress, for a time, by energetic cautery applied along the corneal margin. In order to obtain any result the cautery must penetrate so deeply that all, not only the conjunctival, but also the submucous, vessels are destroyed by it. Wounds from cautery heal easily, and are usually not attended with much inconvenience to the eye. If the tuber has grown to any size all treatment is fruitless. It has repeatedly been attempted to excise parts of the tubers, and cauterize the surface of the wound with nitrate of silver; but the results of the operation have not been encouraging, as the tumours have grown uninterruptedly. The only thing to be done when the tubers are so far advanced in growth that they begin to cover the pupil, which is frequently fixed by films of exudation, is to make a coremorphosis behind the most pellucid part of the cornea. The operation has no influence on the growth of the tuber, and can only be considered as a palliative. Against the development of tubers in the profound layers of the cornea we are completely powerless; operations have been repeatedly attempted, but without avail."

Keratitis punctata, an affection not specially leprous, often accompanies iritis, especially in the tuberous form. Numerous dots, brown or grey, appear in the cornea, of which the lower half is the part usually affected. In time the cornea may again clear, or on the other hand the spots may remain for years without alteration.

The *sclera* is scarcely affected, apart from the infiltrations of the episcleral tissue, which accompany superficial corneal tubers.

Iris.—As in the cornea, the leprous elements are spread over a large surface, or, which happens less frequently, collected into tubers. Traces of iritis are found in 30 per cent. of all leprous patients. In the smooth form the iritis is usually secondary to corneal ulceration after paralysis of the orbicularis, and does not appear till eight or ten years after the commencement of the disease. It is directly due to the dyscrasy in some exceptional cases. In tuberous leprosy, where it is a direct result of the disease, it is as frequent as in the smooth form. It is often chronic; exudations along the edge of the pupil and adhesions to the capsule are not unfrequently found in patients who have not noticed pain or impairment of the sight. When acute, it is usually attended with pain; after severe pain, pericorneal injection, and diminished power of vision. The vitreous is almost always hazy during, or soon after, an acute attack. The haziness usually disappears in some months, though after repeated attacks floating bodies will remain. Both eyes are usually affected.

"The obscuration of the corpus vitreum, observed during the more acute inflammations, shows that the corpus ciliare and choroidea suffer also. Such inflammations are, therefore, to be considered as iridocyclites or iridochoroidites. We have, however, not yet found circumscribed atrophies, pigmentary spots, nor anything like rem-

nants of circumscribed or diffuse choroiditis in patients who have frequently suffered from acute iritis. On the other hand, we have often seen that there has remained a light greyish obscuration of the parts of the retina which surround the optic disc, with a relative tenuity of the retinal arteries. We have noticed in these cases a considerable diminution in the power of vision, even if there has not been any synechia or deposit on the anterior capsule of the lens."

The iris is less liable to tubers than the cornea: those of the former part are not unfrequently accompanied by similar growths in the latter. They always commence at the periphery, usually in the lower half. A partial ciliary staphyloma at the place corresponding to the tuber is not uncommon. The anatomical changes are in all essential points the same as in the cornea. The iritis requires the usual treatment, atropine, morphia subcutaneously, leeches, &c. The tubers may be removed by iridectomy.

Retina.—No pathological change, attributable to leprosy, has been found in the fundus of the eye in more than 200 cases carefully examined by the authors, with the ophthalmoscope. A secondary affection, in the form of white spots near the ora serrata, has indeed been seen in the dissection of some eyes, but only where the iris and ciliary body were much diseased.

The *eyebrow* is a region most liable to leprous deposits. Falling-off of the eyebrows is an early and characteristic symptom of the disease. Tubers in the *eyelid* may be excised when they inconvenience the patient by their position or magnitude.

The *conjunctiva* sometimes becomes infiltrated, when a tuber is rapidly growing in the cornea, but it does not appear to be independently attacked. Paralysis of the orbicularis is a result of leprous disease of the facial nerve. It progresses very slowly, never becomes quite complete, and is accompanied by inflammatory changes in the conjunctiva and cornea in no way different from those produced by other causes. The authors have never met with anæsthesia of the cornea, and do not believe in the existence of the "neuro-paralytic keratitis" said to be found in some lepers. They have had good results in a considerable number of cases of paralytic ectropium, from sewing up the whole inner canthus as far as the punctum lachrymale. Tarsoraphy performed in the usual way does not answer.

The value of the researches of which this is a brief statement may be shown by a few historical remarks. Scarcely anything was known except the simple fact that the eyes were often affected in leprosy, until Boeck[1] perceived that each of the two forms which

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[1] C. W. Boeck, 'Om den Spedalske Sygdom Elephantiasis Græcorum,' Christiania, 1842. We quote from Prof. Fuchs' review in the 'Archiv für die gesammte Medicin,' iv, 268. Jena, 1843.

he recognised was accompanied by a special disease of the eye. He found that the eyes were affected in 49 out of 111 patients suffering from tubercular leprosy. The more usual changes were tubercles at the edge of the cornea, thickening of the conjunctival layer in front of the cornea, maculæ and ulcers of the cornea. There were also noted chronic iritis (six times), cataract (twice); once the eye had burst, twice it was atrophic. On the other hand, in 42 cases of lepra glabra (mutilans, Fuchs, anæsthetica, Robinson) the eyes were frequently implicated; they did not, however, present lesions of the conjunctiva and so on, but ectropium and lagophthalmus (in twenty cases). Ulcers of the cornea, pannus, and specially iritis, both acute and chronic, were also observed.

His statement that in the anæsthetic variety leprous matter is deposited on the spinal cord and about the base of the brain, and in particular that the Casserian ganglion is always affected, has not, so far as we know, been confirmed byother observers. The explanation hence derived, that the lesions are due, in fact, to neuroparalytic ophthalmia (H. J. Vinkhuijzen: geneeskundige opmerkingen op eene reis door het Noorden, p. 85, 94. Leiden, 1865), is, as we have mentioned, rejected by Messrs. Bull and Hansen.

The next considerable advance [1] has been made by the publication of the work under review, in which, as already stated, we meet for the first time with a full and minute account of this subject. It is there shown that the anterior portion of the eye is alone liable to disease directly caused by the deposition of leprous elements; that the latter may be collected into masses (tubers), or spread over a considerable space; that inflammatory changes, obscuration of the cornea, posterior synechiæ, hazy vitreous, often occur, both with and without the deposition of leprous elements, and that they are sometimes due directly to the dyscrasy, sometimes indirectly, as when the eyelids cannot be closed, owing to facial palsy.—T. W.

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[1] Neither an accurate knowledge of details, nor any general view, can be derived from the cases published by Hubsch ('Gaz. Méd. de Paris,' 1854, and 'Ann. d'Oc.,' t. xxxvi, p. 140); Carron du Villards ('Ann. d'Oc.,' vol. xxxvi, p. 145); J. H. Sylvester ('Trans. of the Med. and Phys. Soc. of Bombay,' 1870, and 'Ann. d'Oc.,' vol. lxvi, p. 235); Chisholm ('Ophth. Hosp. Rep.,' vi, 126); Pedraglia ('Kl. Monatstb. f. Aug.,' x, 65). We have found nothing of importance in this respect in the more general essays by A. Fiddes ('Edinb. Med. Journ.,' ii, 1061); J. Y. Simpson ('Edinb. Med. and Surg. Journ.,' vol. lvi, p. 301, &c.; E. Wilson ('Lancet,' 1856, vol. i); Virchow ('Geschwulst-Lehre,' ii); Häser ('Lehrbuch der Geschichte der Medicin, ii, 73, Jan., 1865); Pruner ('Krankheiten des Orients,' p. 163, Erlangen, 1847); 'Report on Leprosy by the College of Physicians,' 1866; R. Liveing ('Elephantiasis Græcorum,' Lond., 1873), &c. See also this 'Review,' vol. xxx (1850), and vol. xlv (1870), and the last edition of T. Fox's work on skin diseases.

Bibliographical Record.

Tholozan on the Plague in Persia, &c.—Dr. Tholozan is well known as one of the best informed and most enlightened epidemiologists of the present day. During his long residence at Teheran, as principal medical officer of the Shah, he has most assiduously watched the course of the principal epidemics which have of recent years occupied professional attention; and, being a good scholar, his stores of information have been derived from very ample and varied sources. We had lately occasion to refer to his labours in illustration of the past career of cholera, and we have now to draw attention to what he has done in respect of certain local outbreaks of the true Oriental plague, the history of which has given rise to no small discussion among medical men throughout Europe in consequence of their bearing on some intricate questions of State medicine.

Like all other scientific physicians, he attaches the utmost importance to the study of geographical chronology in the investigation of the origin and diffusion of epidemic diseases, and in the due appreciation of all attempts at their prophylaxis or arrest by hygienic and sanitary appliances.

The marked unsuccess of most governmental and international medical congresses has been mainly owing, he thinks, to the neglect of this indispensable branch of preliminary inquiry, and to the members of these commissions proceeding at once to frame practical conclusions on various topics respecting which their authentic information is always meagre and often uncertain. In 1869, Dr. Tholozan published a memoir on the outbreak of plague which occurred in 1867 in the environs of Bagdad, after an apparent disappearance of the pestilence throughout the Ottoman empire since 1843-4, when it last prevailed at Erzeroom and other places in Asia Minor. His researches showed that the development of the disease had been by no means so sudden and unexpected as is

[1] 1. *Histoire de la Peste Bubonique en Perse.* Par J. D. THOLOZAN, de la Société Epidémiologique de Londres. 8vo, pp. 42. Paris, 1874.

2. *Histoire de la Peste Bubonique en Mésopotamie.* Par J. D. THOLOZAN. 8vo, pp. 91. Paris, 1874.

generally imagined, but that for many months previously there had been a notable tendency in the ordinary endemic fevers of the locality to being complicated with bubonic affections of the axillary and inguinal glands. "La petite epidémie de peste de la Mésopotamie en 1867 fut précedée de phenomènes pathologiques importants rappelant ces constitutions médicales pestilentielles qui dans le seizième et le dix-septième siècle signalerent quelquefois l'arrivée des grandes pestes dans certains pays d'Europe." A still more interesting result is pointed out, viz., that a similar medical constitution prevailed in some districts of that region during 1857-8, when the partial and local outbreak of plague occurred at Benghazi in Western Barbary. Such synchronism of epidemiological phenomena in countries remote from each other, if well authenticated, is obviously of extreme interest in the genetic history of spreading diseases, and is in itself sufficient to attest the vital importance of medical geography and chronology in ætiological studies.

The unscientific conclusions of the Ottoman Board of Health in respect of the Mesopotamian outbreak of 1867 are criticized with just severity by our author. Notwithstanding the recorded distinct opinion of their own medical secretary, after a personal examination of the disease in the affected districts, the Board sitting at Constantinople decided that it was not genuine bubonic plague, but only a "pernicious paludal fever" accompanied with glandular swellings,—because the outbreak had not been "envahissante," and did not manifest strongly contagious properties!

Dr. Tholozan's two recent *brochures* give an instructive record of all the successive manifestations of the plague in Persia and in Mesopotamia, as accurately as the very imperfect published data on the subject enable him to do so. Together, they form a most valuable contribution to the history of the pestilence in these regions of the East. The evidence he adduces distinctly controverts many of the statements made by Dr. Bartoletti, the head of the Turkish sanitary authorities, in his late official report to his government "sur les mesures à prendre contre la peste qui sevit en Perse," which appears to have given rise to voluminous diplomatic correspondence on the part of France, England, and Russia, with Turkey and Persia. Now that the relations of Quarantine in reference to cholera have been so thoroughly modified by the decisions last year of the Vienna International Congress, it may be reasonably hoped that the question of the proper restrictive measures against the two other diseases against which quarantine has been mainly directed—plague and yellow fever—will, ere long, be thoroughly investigated by some competent authority, to the benefit of humanity as well as of scientific truth. To one point alone, bearing as it does not only on the above question, but also on the large and intricate problem of the genesis of various fevers,

domestic as well as foreign, we shall allude, and that is that in all the local outbreaks of plague, which have occurred during the last fifteen years, the pestilence seems to have sprung up in the districts affected as a *de novo* development, quite unconnected with any discoverable existence of the *materies morbi* from antecedent cases. Dr. Bartoletti distinctly declared, in his account of the Benghazi outbreak in 1858, that "cette peste est née spontanément." Dr. Naranzi, describing the epidemic near Bagdad in 1867, expressed his opinion that it "naissait spontanément dans deux endroits peu distants l'un de l'autre;" and Dr. Castaldi uses nearly the same language in reference to the partial and scattered outbreak in Persian Kurdistan in 1871. Medical men will differ as to their acceptance, or otherwise, of these conclusions. Only it behoves them all to acquaint themselves with the ascertained facts of these several cases, and not determine the point in their mind under bias or prejudgment. No question of scientific nosology more strongly demands a thorough impartial scrutiny.

Postscript.—Since the preceding remarks were written, we have received the official account[1] of the late reappearance of Oriental plague in that part of Barbary which was the seat of the unexpected outbreak in 1858. The earliest cases occurred about the beginning of April in two Arab tent-encampments near the village of Medje, which is eighteen or twenty hours' march inland from the coast town of Benghazi in the pashalic of Tripoli. It is believed that sporadic single cases had been seen among the wretched inhabitants for several months previously; but nothing accurately is known. Dr. Laval, who subsequently fell a victim himself, was the first to determine the true nature of the fever, that it was genuine bubonic plague, various contradictory statements having been circulated about it. Dr. Arnaud did not reach the spot until the fever had begun to decline in Medje, and after it had attacked several places in the neighbourhood. Nothing could be more thoroughly unsanitary than the foul abodes of the poor people, or more miserable than their hygienic condition in respect of food and clothing. For four or five years before 1874, there had been a wasting famine, just as had been the case for some years prior to 1858, so that the inhabitants had been reduced to absolute want, compelling them to resort to the use of the most repulsive objects to satisfy the cravings of hunger. In both instances, the harvest in the plague years was more abundant, in consequence of the heavy rains which had preceded and caused the prolific vegetation. Dr. Arnaud thinks that the great humidity of the ground and of the atmosphere thus occasioned had something to do with the genesis of the fever which

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[1] *Essai sur la Peste de Benghazi en* 1874. Rapport du Dr. L. ARNAUD, publié par l'Administration Sanitaire Ottomane. Constantinople, 1875. 8vo. Pp. 62.

sprung up among a population predisposed to its attack by their previous indigence and starvation. He is convinced that *it was of spontaneous origin*, as Dr. Bartoletti was in 1858 in respect of the outbreak that year.

It is a most interesting epidemiological fact that North Africa was not the only region where plague manifested itself in 1874. On the authority of the Ottoman administration, we learn that "peu avant cette époque la peste était signalée en Mésopotamie (Hindié, Divanieh), et des bruits couraient qu'une maladie *analogue* existait dans l'Assyr (Yemen)."

The fever appears to have been by no means widely spread in the district near Benghazi, where it originated, and to have gradually declined and become extinct by the adoption of simple sanitary measures. That quarantine restrictions had anything to do with its subsidence seems extremely doubtful.

Erichsen on Hospitalism.[1]—Both the subject and the author of this work deserve special attention; the former because it has been a matter of much discussion in both professional and general circles, and the latter because he is not only an eminent surgical authority, but also because he is almost a specialist on the subject on which he writes.

The lectures are so full of thought and so suggestive of thought, and so decisive in their teaching that they must produce or tend to produce one result, that is, the extinction of the word hospitalism, as expressive of a *necessary* condition. The unhealthiness of hospitals will ever make the general body of the medical profession look upon hospitals as at present managed as not nearly so healthy as private houses. Lectures such as these which acknowledge the evil and point to certain rational remedies, must, however, tend to the great practical result desired by all, viz. reducing the mortality from septic diseases.

In Lecture I we are told—

"Surgery in its mechanical and manipulative processes, in its art, in fact is approaching, if it has not already attained to, something like finality of perfection. The science of surgery has not advanced proportionately with the art."

All acknowledge the great advances made in surgery since the introduction of anæsthetics, and of the principles of conservative surgery. Yet there is still much to be done, especially in the treatment of cicatrices after burns, of congenital malformations, of stone in the bladder, and of various forms of pelvic fistula. Moreover, so long as we rarely get primary union, which usually depends on a proper adaptation of surfaces, we cannot look upon our art as

[1] *Hospitalism and the Causes of Death after Operations and Surgical Injuries.* By J. E. Erichsen, F.R.C.S., &c. London, 1874.

perfect. The gist of the whole book, however, is really comprised in the second statement, viz. that the science of surgery has not advanced proportionately with the art. The rate of mortality has not decreased in anything like the same proportion as the art has progressed; the cause of this mortality is chiefly what has been called hospitalism; the cause of the hospitalism is overcrowding of cases and want of sanitary precautions; the remedy for this want of success is hygiene in its widest sense.

Many do deny that there is any such condition as hospitalism; whether there is or not matters very little; the fact remains, that in ordinary hospitals maternity wards have been abolished on account of the high rate of mortality. Ovarian cases are almost always fatal; pyæmia, septicæmia, and erysipelas are fearfully common; something within the hospital must be the cause of this state of things, and it is only rational to suppose that, unless the cause be partially or entirely removed, many surgical wards will follow the same fate as maternity wards and be swept away.

Has anything been attempted or done to remedy this state of things? Speaking generally, we must distinctly say, no! Individual surgeons, much to their credit, have attempted and done much; but the majority of surgeons have sought to ignore facts, and have thrown cold water on attempts at a theoretical and practical solution of the difficulty.

For example, Prof. Lister's antiseptic treatment has been before the profession for several years, yet it may be asked how many hospital surgeons understand it, and how many have fairly tried it? The number could be almost counted on one's fingers; yet this method of treatment professes to do away entirely with all risk of hospitalism or hospital plagues. Professor Lister and others have published numerous cases treated by this method, and in the 'Edinburgh Medical Journal,' for March, 1875, Professor Volkmann, of Halle, contributes a remarkable paper on antiseptic osteotomy, and shows the result of antiseptic treatment in his hands to be most successful; he says—

"Since the introduction of the antiseptic method into my clinique now exactly two years ago (at the end of November, 1872), no single patient suffering from a compound fracture in which conservative treatment was attempted, has died. Amongst this number are included even those cases in which conservative treatment was only resorted to, because the patients would not give their consent to amputation, and also in which we at first under-estimated the severity of the injury, and afterwards intermediate or secondary amputation had to be undertaken on account of hæmorrhage or gangrene. The number of compound fractures successfully treated without a single fatal result in our hospital, which is old and always overcrowded, and offers the most unhealthy hygienic conditions, amounts at

present to 31; amongst these were as many as 19 compound fractures of the leg, in several instances much comminuted, and often complicated with most severe bruising and laceration of the soft parts. There were also two compound comminuted fractures of the patella, both of which recovered with movable joints. No case of pyæmia has occurred for a year and a half, *i.e.* since July, 1873, although during this period alone about 60 major amputations have taken place."

The paper contains other passages quite as remarkable in showing the great value of antiseptic surgery in hospital practice.

Seeing, then, how the expectant method (if one may use the term) in the treatment of injuries and wounds in hospitals has failed, and how high the mortality is, it appears only a rational course to try other methods, and especially the plan advocated by Lister, Erichsen, Callender, and others. Hygiene in relation to surgery means a large amount of attention to details in the after-treatment of wounds, and when this amount of attention is bestowed the surgeon is usually said to have a "hobby" for that kind of thing.

Holden on the Sphygmograph.[1]—Every new instrument of clinical research has to contend with two obstacles which retard its introduction. These are, the over-zeal of its friends, and the prejudice of the conservative or self-styled practical members of the profession. The history of the sphygmograph since its introduction by Marey illustrates the rule. The exaggerated notions which were formed of its value have given practical opponents plenty of arguments for a critical disbelief in its usefulness. If the sphygmograph had proved the ready means of diagnosis in heart disease, and the infallible indicator of the state of the circulation in fever, which some observers announced, its invention would have been a very doubtful advantage. It will, indeed, be a sorry day for medicine when any instrument is devised which will do away with the necessity for careful observation and cautious induction. We do not therefore regret that the value of the sphygmograph was overstated, and that a more sober view of its powers now prevails. That it will eventually prove a great boon to the physician, we have no doubt, and the day will come when objections based on the obscurity of its hieroglyphics will be refuted by the lucid translation of all forms of pulse trace. The time was, when many of the revelations of the stethoscope were unintelligible, and the records of the thermometer still often have a doubtful significance, but on this account few would question the value of these instruments. The same patient work which has made these the

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[1] *The Sphygmograph: its Physiological and Pathological Indications.* The essay to which was awarded the Stevens Triennial Prize, by the College of Physicians and Surgeons, New York, April, 1873. Two hundred and ninety illustrations. By Edgar Holden, A.M., M.D. Philadelphia, 1874.

almost inseparable companions of the practitioner will in time establish the value of the sphygmograph. The chief impediment to progress hitherto has arisen from the attempt to make the sphygmograph a measurer of arterial tension. This, it is only in a minor degree. It may serve as a comparatively rough test of arterial fulness, but its great function is to record the forms of arterial movements. The attempt to perfect it as an indicator of tension has led to the numerous modifications which have been made in the form of the instrument. Hence has arisen that confusion in the work which is necessarily the result of having a large number of observations which are not strictly comparable. The author of the book before us is a sinner in this respect. Marey's sphygmograph (which, with very slight modifications, is in our opinion the best) appeared to Dr. Holden to be faulty from not registering the displacing power of the artery as well as its lifting power. He therefore invented a form of pulse spring to partially surround the artery by means of a concave surface. Each pulsation transmits to this spring a lateral as well as an upward motion, and the result is recorded by a lever moving laterally instead of vertically, as in other sphygmographs. The second fault which Dr. Holden sought to remedy by his invention was the mode of application; all the usual means of fixing the sphygmograph appeared objectionable, so he abolished them, and simply holds his instrument in position by the thumb and finger. Whatever value there may be in the altered shape of the pulse spring, and we cannot estimate this highly, the method of application is in our experience sufficient to vitiate the results. Any one who has tried an ordinary sphygmograph by holding it over the artery, knows how the vibrations of the hand of the observer will spoil most of the tracings. It may not be so to the same extent with Dr. Holden's instrument, but that it is so to a great extent the numerous pulse traces in his book prove by their irregular lines. We have ourselves collected many tracings vitiated by the same cause.

The book is divided into three parts. The first treats of the mechanism of the sphygmograph and the pulse; to this part we have alluded sufficiently; the second part considers the special application of the instrument to the study of pathological conditions; and the third, its value in therapeutical research. A large number of traces taken in disease are well and clearly printed in the plates, but we cannot say that they are worthy of a prize essay. Many of the tracings are so badly developed, and show so few of the generally recognised elements of the pulse-trace, that they are of little value. Others, which are more worthy of study, are spoiled by the extremely meagre and often vague record given in the text of the cases which they illustrate.

In the therapeutical inquiries we are almost equally disappointed. The most noteworthy point is the large quantity of Cannabis Indica

taken by the author—420 drops of the tincture—on one occasion in the course of three hours, and in a second observation 26 grs. of the fresh alcoholic extract in the same time. The physiological effects in the first instance appear to have been very slight, and in the second experiment neither the sphygmographic nor the subjective phenomena were in proportion to the courage of the self-tormenting observer. Other experiments were made with aconite, gelseminum, and quinine, but with little addition to our knowledge.

We are sorry not to be able to congragulate the author on his work. His great misfortune was, we think, the invention of a new form of sphygmograph. With one of the original instruments of Marey, his patience and industry would, we believe, have made more important additions to the literature of the sphygmograph.

Dunglison's Medical Dictionary.[1]—Dunglison's dictionary has acquired an honorable place in medical literature, and very deservedly so by reason of its many merits. A new edition was much needed on account of the vast multiplication of terms used in medicine and the collateral sciences, invented by actual or supposed discoverers, and particularly in Germany. We are, therefore, not surprised to hear that "the present edition will be found to contain many hundred more terms than the first, and to have experienced numerous additions and modifications." Nevertheless, it would be possible for the student versed in Teutonic and French medical literature to find not a few terms missing, in sufficient vogue to deserve a place. A defect of this kind is, however, inevitable in any such compilation, and when the medical pupil has a volume like the one before us placed in his hands as constituting a mere lexicon of terms or skeleton of facts that he must transfer to his head and clothe with abundance of knowledge, he may well cry, hold! enough!

The author states that his "object has not been to make the work a mere lexicon of terms, but to afford under each, a condensed view of its various medical relations, and thus to render the work an epitome of the existing condition of medical science." This object we regard as too ambitious and extensive for a work such as Dr. Dunglison had in view, or was really demanded. An epitome of the shallowest dimensions representing the actual condition of medical science, together with that of the several collateral branches of that science, as comprehended in this volume, is beyond the compass of a dictionary like that before us, and beyond the power of most ordinary mortals to produce. And, in fact, the necessities imposed upon the author have of themselves saved him, in regard to

[1] *A Dictionary of Medical Science with the Accentuation and Etymology of the Terms and the French and other Synonyms.* By ROBLEY DUNGLISON, M.D. A new edition enlarged and thoroughly revised by RICHARD J. DUNGLISON, M.D. London, 1874. Pp. 1131.

most terms, from his ambitious project; and where he has ventured on further detail than is necessary to explain the terms before him, he is often at fault in introducing obscure and ambiguous information, doubtful opinions, and questionable practice. His notes of treatment in various important diseases are uncalled for, and are generally open to strong objections. As a matter of course they are very brief, and so far as they apply to disease expressed by some general term, though comprehending pathological conditions of great variety, they would, if acted upon, as a rule, prove positively mischievous. Active antiphlogistic measures, blood-letting to produce a decided effect, antimonials and kindred agents, are prescribed, accompanied by few provisoes, in most inflammatory affections.

The definitions of anatomical terms might, in various instances, be much amended as regards perspicuity at least, if not correctness. The classification of the human races, introduced under the heading *Homo*, need revision, for that of Blumenbach will not pass muster at the present day among ethnologists.

Another desideratum of this dictionary, now that it has assumed the character of an English publication, issued by our great London medical publishing firm, is that, the formulæ of the British Pharmacopœia should find a place alongside those of the United States Pharmacopœia, which, in the present edition, furnishes the basis of reference for the pharmaceutical preparations enumerated.

Although we have thus freely spoken of shortcomings, we are fully sensible of the excellencies of this medical dictionary, and these in themselves are sufficient to secure for it the good opinion of the profession, and, as we trust, its success as a publication.

Manual of Public Health for Ireland.[1]—Division of labour is usually valued as a means of economizing time and facilitating production. The principle has been applied in the production of the work before us; clearly, however, not for the sake of the end commonly aimed at in the way of rapid development, for the preface apologises for delay in execution, but on the assumption presumably that the special matters comprised within the scope of public health legislation can be best treated by those specially informed in them. It is thus we account for the strong force of four writers in the production of this small treatise, viz., of two medical doctors, a barrister, and a professor of chemistry. At the same time, viewing its contents, we must say that the array of talent seems much in excess of the demand for it. The sections relating to legal requirements are extracted, as a matter of course, from the provisions of "The Public Health (Ireland) Act, 1874," and, as for other portions, they are gathered

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[1] *Manual of Public Health for Ireland.* By Thomas W. Grimshaw, M.D., &c.; J. Emerson Reynolds, F.C.S.; Robert O'B. Furlong, M.A.; and John W. Moore, M.D., &c. Dublin, 1875.

from well-known and esteemed authorities on the several subjects dealt with. In short, we feel persuaded that the whole matter might have been readily elaborated by the general editor, Dr. J. W. Moore, who has made public health a special study, and is well known as a medical meteorologist.

The volume is divided into no less than twenty-seven chapters, of which the first four, presenting an analysis of the Health Act, a digest of the laws prescribing the duties of sanitary officers, a list of statutes relating to public health in Ireland, and an index to the sanitary acts comprehended within that list, occupy nearly a fourth of the whole; consequently the many other subjects undertaken get very slenderly treated, as may be supposed when we say that, besides chapters on vital statistics, on births and deaths, on population and the development of man, an attempt is made to examine, in connection with sanitary science, zymotic diseases; special zymotics; diseases arising from insufficient or unwholesome food; diseases usually termed constitutional; artisans' diseases; diseases caused by vicious habits; food; water supply; examination of water; house construction; air and ventilation; sewage and its treatment; contagion and disinfection; accommodation for the sick; meteorology and climate; meteorological observations, and influence of season on zymotic diseases. However, it is due to the several writers to state this much, that the information conveyed is very clearly and succinctly given, even if it affords but a rude outline. In proportion to the extent of consideration devoted to other subjects, meteorology and climate have received a larger share, and so far may be regarded to approach adequacy of treatment.

Although the profession has been well supplied with manuals of sanitary medicine, and the general laws of health are alike in England and in Ireland, a reason for the publication of a manual for Ireland specially is to be found in the particular provisions of the "Public Health (Ireland) Act, 1874;" notably in the allotment of the functions of sanitary medical officers to the dispensary medical officers of the country, and in the designation of the local authorities entrusted with the carrying out of the Act. We doubt not, therefore, that this treatise will be appreciated by our Irish professional brethren. And, indeed, it is well worthy of their good opinion, for if the information conveyed be too largely superficial, it is both accurately and well given.

Aitken's Outlines of Medicine.[1]—This work is expressly designed for the use of students, and aims at furnishing a synopsis of the most important facts regarding the natural history of diseases, and the lines of treatment by which remedies are to be applied for the pre-

[1] *Outlines of the Science and Practice of Medicine.* By WILLIAM AITKEN M.D., F.R.S. London, 1874.

vention or the curative management of individual diseases. Such are the purposes of this volume as set forth in the preface; the reputation of the author will be a guarantee that those purposes are faithfully and efficiently fulfilled. Its character as a digest of admitted facts and accepted doctrine, and as a text-book for students, limits the task of the reviewer to simply recording his opinion of the completeness and correctness of the matter contained in it, and of the manner in which the matter is conveyed. Now in respect of both matter and manner our examination of the treatise has convinced us that nothing could be better. If fault must be found with the book, it consists in the concentrated character of the material. It represents the very essence of physic, and nought but the strongest digestive powers could continuously make use of and duly appropriate it. For the special purpose of upholding the student through the febrile heat of examinations, it is admirably suited; but at other times, the learner would desire more diluted material, rendered also more palatable by a commixture of practical remarks and of case narratives.

To refer to one particularly commendable feature, we would remark on the amount of valuable information conveyed in the sections treating "of the methods of clinical investigation introductory to those diseases which are described in groups under the organs or systems to which they respectively belong." Indeed, we know of no summary of the use of electricity as a means of diagnosis equal to that contained in the "guide to the clinical investigation and diagnosis of diseases of the nervous system."

C. Fox on Water Analysis.[1]—This work is the substance of a paper read by its author in the Public Medicine Section at the Norwich meeting of the British Medical Association in 1874, and it is stated in the preface that, in the discussion which followed the reading, the recommendations of the paper were endorsed. This statement forms a very doubtful recommendation of the book, seeing how very few medical officers of health are competent to form an opinion as to the value of methods of chemical analysis. Indeed, it may be doubted whether any appreciable number of these officials have had the opportunities of instituting comparisons between the method adopted by the author and other rival methods. We fancy, too, that the medical officers of health must have been taken with the assertion of Dr. Fox that "a *complete* analysis, then, of a potable water can with practice be accomplished in forty minutes" (the italics are Dr. Fox's). Nay, further, he states that the time occupied in answering the question, "Is this water wholesome and good?" can be answered with absolute certainty in thirty minutes.

[1] *Water Analysis as it should, and as it should not, be performed by the Medical Officer of Health.* With illustrations. By Cornelius B. Fox, M.D., M.R.C.P., Lond. London.

If these statements could be substantiated, water-analysis would be a very simple affair indeed, and all the fuss made in these days about wholesome and unwholesome water would be misplaced.

The author of the brochure before us states that it was written with the following praiseworthy objects:—(1) For the purpose of inducing all *real* medical officers of health to adopt some one reliable method of water-analysis, so that the results of the examinations of all might be comparable; (2) to demonstrate to them the superiority of the Nessler process to any other; and, (3), to give to them some of the results of his experience in the examination of potable waters. The process on which Dr. Fox relies for the determination of the wholesomeness or unwholesomeness of water is what he terms "the Nessler process," by which he means the process devised by Chapman and Wanklyn, though the author does not state this. Indeed, his pamphlet is substantially a reproduction of their treatise, and he inculcates theirs as the one reliable method of water-analysis. We are not prepared to deny this to be the case, but, nevertheless, think that Dr. Fox is not happy in demonstrating this mode of water-analysis as superior to all others; in fact, he merely shows that the popular methods of smelling a water, treating it with permanganate, and testing for nitrates, are not reliable tests for organic contamination. It would, therefore, have been well not to have attempted "to demonstrate the superiority of the Nessler process to any other." No mention whatever is made of Frankland's well-known process of estimating organic matter in waters. We note, however, with satisfaction that all Mr. Wanklyn's dicta are not accepted; and notably this, that a considerable quantity of free ammonia in a water containing ·005 per 100,000 of albumenoid ammonia is to be regarded with suspicion. Dr. Fox quotes good waters which contained ·007 of ammonia; but he might have gone further, since it is notorious that some of the excellent deep artesian well waters of the London basin afford seven or eight times as much free ammonia as he quotes.

Notwithstanding these defects, and some minor blemishes, we can recommend the work before us to medical officers of health, as giving an outline of a method of determining the organic matter very commonly adopted. The *modus operandi* is, on the whole, clearly described. We must, nevertheless, protest against the advice which Dr. Fox gives to operate, for the sake of saving time, upon minute quantities of water, *e.g.*, 25 cubic centimètres for the determination of total solid residue. With such a quantity, and even with an accuracy of weighing beyond the skill and appliances of a medical officer of health, great inaccuracy of results may result.

It would be well for the author in a future edition to rewrite the section of his work relating to hardness of waters. Besides a misprint of "temporary" for "total," the mode of calculating the quantity of carbonate of lime, or other salts equivalent in hardness

to carbonate of lime, contained in the waters is inaccurate, and the mode given for estimating the permanent hardness of a sample of water (a most important operation) is too indefinite to be of any value.

C. Fox on Disposal of Slop Water.[1]—This handy little pamphlet appears most opportunely, and will form a welcome addition to the library of every rural medical officer of health. Since the more general adoption of dry systems of disposing of excrement, great difficulties have attended the disposal of slop water, and it is the endeavour of Dr. Fox to show how these difficulties may be overcome. It may not be superfluous to state that slop water consists of soapy water, rain, dirty water, and a small quantity of urine, but where a dry-earth system is carried out slop water is not mixed with solid fæcal matter. Slop water has in consequence a minimum of manurial value, and as a matter of £. s. d. is valueless. It is obviously inexpedient to discharge slop water into watercourses, and although such stuff is not sewage—a term which implies fæcal matter—and hence may legally be discharged into a stream; the Local Government Board will not sanction the discharge of slop water into watercourses. Its disposal without nuisance is consequently a matter of great importance, and often one of great difficulty.

Dr. Fox describes six different and simple methods of disposing of slop water other than its discharge into streams, and the cases must be rare, indeed, where one of these methods is not available. For the description of these methods, we must refer to the work itself, which is one that ought to be in the hands of every medical officer of health. We can cordially recommend its perusal to all who have to do with any administration of sanitary law. It forms a small but valuable contribution to sanitary literature.

McCall Anderson on Eczema.[2]—The opening chapter of this handsome volume, which discusses the character and pathology of eczema, is, as the writer admits, a reproduction of the doctrines of the Vienna school. It is from what all younger English and American dermatologists have learnt, either directly or indirectly, from Professor Hebra, that our present knowledge of the disease differs from the views "which previous to the first edition of this work were adopted and taught by the majority of the profession in this country." Twenty years ago Mr. Erasmus Wilson was the only man in the

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[1] *The Disposal of Slop Water of Villages.* By Cornelius B. Fox, M.D., M.R.C.P., Lond. London, 1875.

[2] *A Practical Treatise upon Eczema, including its Lichenous and Impetiginous Forms.* By Dr. McCall Anderson. Third edition, with seven woodcuts. Pp. 208. London, 1874.

United Kingdom who knew much more of diseases of the skin than what had been taught by Willan and Bateman. Since then Mr. Hutchinson, Dr. T. Fox, Dr. Fagge, Dr. Buchanan, and Dr. Anderson himself, with many other good workers, have made exceedingly valuable contributions to the subject. But, on the whole, modern dermatology owes to Germany its present "pathological" aspect, in distinction from the "symptomatic" study of eruptions of the older British school, and the "diathetic" theories of French writers. At the same time we think that Dr. Anderson is unfair to Willan and Bateman, and fails to apprehend the true objects of classification when he writes:

"Defective as any classification of skin diseases must be, there can be little doubt that the anatomical classification is the most objectionable of all; for in this way many dissimilar diseases are brought together under one group, while violence is done to the symptomatology of many of them, owing to the necessity of placing them under the head of one of the elementary lesions. Thus scabies is ranked with ecthyma and smallpox—diseases which have no connection with one another whatever; and the first of these (scabies), though it often shows itself in a pustular form, is still more commonly met with as a vesicular or papular eruption, or as a mixture of all three."

There is no necessity for a classification of skin-diseases to be defective, except from deficiency of knowledge. What *is* unavoidable is that no single classification can exhibit all the likenesses and differences of these or any other diseases, for the simple reason that a "disease" is not a natural object, but an artificial and complex conception, including a material cause, a physiological state, and an anatomical structure, together with such psychological conditions as pain or itching; while even to these we must, if we follow all ancient and some modern teachers, add such metaphysical *idola theatri* as occult terrene miasms, impurities of the blood, strumous diatheses, dartrous dyscrasiæ, and lymphatic temperaments. But if the classifier does not attempt to classify dissimilar objects, there is no difficulty in grouping cutaneous like any other diseases, according to whatever feature may be selected. Thus, choosing etiology as a basis, we have a natural class of diseases dependent on animal parasites, another on the presence of fungi, a third on syphilitic infection, a fourth on poisoning by copaiba and other irritants. Or, taking a pathological ground, there is no difficulty in distinguishing acute and chronic inflammations, congestions, hæmorrhages, atrophies, tumours, &c. If this classification is combined with an arrangement according to the tissue affected, into diseases of the horny cuticle, of the malpighian and papillary layers, of the deep cutis and subcutaneous tissue, of the sweat-glands, sebaceous sacs, hair-follicles and nails, there results the classification adopted by

Hebra from Rokitansky, which is perfectly satisfactory and useful *for the objects intended.* Quite independent of this and of great practical use is a classification by locality; all diseases of the scalp have important points of similarity; so have those of the face, and those below the knee. The thin skin of the fingers and toes, the prepuce and the nipple has a peculiar liability to scabies, the flexor surfaces to eczema, the extensor to psoriasis, the palms and soles to syphilis. Again, an instructive arrangement of cutaneous diseases may be made according to their reaction to remedies — those benefited by soothing applications, by astringents, by caustics, and those again which are almost unaffected by local treatment, but disappear under mercury or arsenic. Just in the same way we may classify diseases of the lung by their origin as tubercular, parasitic, syphilitic, or accidental; by their pathology, as inflammatory, atrophic, cancerous, and so on; by the tissue affected, the bronchia, the vesicles, the connective tissue or the pleura; and locally as they affect one lung or both, the apex or the base.

Now, the anatomical classification of Willan and Bateman is only objectionable when used exclusively and for purposes of diagnosis; a knowledge of the "elementary lesions" of the skin is as necessary as a knowledge of the elementary physical signs of disease of the lungs. A student must first be taught to recognise a papule, a scab, or a vesicle when he sees it, just as he must be taught to recognise a rattle, a rub, or a crepitation, when he hears it. Scabies, ecthyma, and smallpox have just this connection, that each may produce pustules; as cancer of the lung, pneumonia, and phthisis may each produce bronchial breathing.

After the paragraph above quoted Dr. Anderson goes on to say:

"There can be no doubt in my mind that the best classification of skin diseases is one founded not upon the elementary lesion, but, as far as possible in the present state of our knowledge, upon the nature of the affection. This is the basis of the classification of Hardy, and that which was adopted some years ago by my late colleague (Dr. Buchanan) and myself."

But we neither know, nor are likely to know, anything of the "nature" of diseases; we must be content to study their anatomy, symptoms, and causes. Alibert's showy and shallow "system" remains a warning against premature attempts of the kind, and dermatology only gained a solid basis in France when Biett introduced the "objectional" classification of Willan. Dr. Buchanan's classification, above referred to, into erythematous and phlegmonous inflammations, new growths, and so on, was made on a pathological basis. Mr. Hardy's, into dartres, scrofulides, syphilides, &c., is essentially etiological, and each is valuable for a different object. The study of dermatology will never become scientific till it ceases to be a

specialism, till systems and classifications are recognised at their true value, and we no longer label every lesion with a generic and specific name, as if it was a plant. Diseases of the skin are, like those of any other organ, very complex phenomena, to be studied under many different aspects, in their anatomy, causation, symptoms, and treatment, but by the same methods and principles which have introduced scientific knowledge into the study of tumours or of fevers.

Dr. Anderson follows Hebra in recognising the fact that the *elementary lesion* of eczema is not always and of necessity a vesicle. It may be a mere erythema, or papule, or pustule. Hence the diseases commonly known as lichen and impetigo are mere varieties of eczema, when they are not syphilis or scabies. The *symptoms* regarded as essential are infiltration of the skin, exudation on its surface, formation of crusts, and burning or itching. The form described by Hardy as "eczème fendillé," and by Dr. Fox as "eczema fissum," Dr. Anderson prefers to call eczema rimosum. As to *pathology*, our author agrees with Rindfleisch and Fox in calling the disease a catarrhal inflammation of the skin. The affection, however, which was described by Hebra as eczema marginatum, is, at least in most cases, a local variety of tinea, as shown by Köbner and confirmed by two excellent cases related by Dr. Anderson. The following chapter, on the *histology* of eczema, is illustrated by drawings from Biesadecki, Neumann, and Rindfleisch. The *causes* of the disease, apart from local irritation and hereditary transmission, are quite unknown. It is, we think, a pity that so eminent a physician as Dr. Anderson should be content to repeat such vague and contradictory remarks as the following:

"It attacks by preference the rachitic, the scrofulous, and the debilitated; indeed, it may be safely affirmed that debility, in some form or other, lies at the root of a very great number of cases of eczema. At the same time it must be confessed that it often attacks persons who are apparently in the most robust health, in whom neither external nor internal causes are apparent. These cases must be referred to some idiosyncrasy—the dartrous diathesis, as the French call it, which is certainly a convenient word to cloak our ignorance of its nature."

Why reasonable men must refer diseases to an idiosyncrasy or diathesis, and why honest men should wish to be thought wiser than they are, we do not understand.

Dr. Anderson remarks that the skin of persons who are subject to eczema is darker than natural; that its natural lines and furrows are deeper and wider apart than in health, and that there is more or less desquamation. In 500 consecutive cases treated by him at the Glasgow Dispensary, 296 occurred in men and 204 in women. This agrees with the experience of Hebra and of Mr. Wilson.

Eczema mercuriale is put among the forms due to local irritants, Dr. Anderson having never seen it occur from the internal administration of mercury. A useful chapter follows on the differential *diagnosis* of eczema from (non-eczematous) erythema, from erysipelas, herpes facialis or preputialis—the latter is actually called a variety of eczema by Hardy—scabies, psoriasis, pemphigus foliaceus, pityriasis rubra, lichen ruber, and syphilis.

Dr. Anderson agrees with Hebra in regarding the supposed danger of curing an eczema as a mere fancy, and quotes two cases of the disease being masked by an attack of measles, to reappear when it passed off. Under the head of *treatment* he discusses at length the benefits derived from purgatives, diuretics, steel and cod-liver oil, where indicated by the early and acute stage of the disease, by "torpidity of the kidneys" or "excessive acidity of of the system," or by emaciation and pallor. In chronic cases without special indications, he recommends *strychnia* and *arsenic*—the latter in increasing doses, which may occasionally be pushed to ten, fifteen, or even twenty minums of Fowler's solution three times a day, with impunity and with benefit. Alkalies and sulphur Dr. Anderson considers of little use, but in chronic cases he has seen good results from the internal exhibition of tar, though not equal to those obtained in psoriasis. In prescribing arsenic or strychnia, the following excellent rules are given:—1. Begin with a small dose, and increase it until it disagrees or cures. 2. If it disagrees, try another form, or wait a few days and give it again. 3. Give it a fair trial for several weeks. 4. When the patient is cured, do not stop the medicine, but let him leave it off gradually.

As to local treatment, Dr. Anderson advises, first, the various preparations of potash introduced from Vienna by himself and Dr. Buchanan; and next in importance, especially for chronic cases, ointments, soaps and emulsions, containing tar, juniper oil, carbolic acid, or similar drugs. Some interesting cases are given of treatment by the impermeable dressings of vulcanized india-rubber, first used by M. Colson of Beauvais, introduced thence by Hardy into St. Louis, and thence again by Hebra to Vienna (see report in the 'London Medical Record' for June 9, 1875). Dr. Anderson thinks that this mode of treatment will prove an important addition to our means of combating eczema. The book concludes with a careful and practical account of special points in the diagnosis and treatment of the local varieties of eczema, with tabular statements of the differences between eczema of the scalp and syphilis, psoriasis, or ringworm, attacking the same region; and between eczema of the beard, and ringworm or acne of the part, which are often confounded together as sycosis.—P. S.

Mapother on Skin Diseases.[1]—These lectures first appeared, after delivery, in the 'Medical Press,' and were subsequently collected in a small volume. Their recommendation is their brevity, coupled with good practical information respecting the leading pathological features, and the treatment of the diseases described. The note on the title-page, "with illustrations," will mislead the reader who may, in consequence of it, anticipate figures illustrative of skin eruptions, for the illustrations given consist solely of a few rough woodcuts intended chiefly to portray cutaneous parasites. If, as is probable, the success of this little treatise encourage Dr. Mapother to issue a further edition, or to embark on a work of greater dimensions, we would suggest to him the propriety of more attention to composition, inasmuch as that of the present volume is very slipshod and too often ungrammatical.

Moore's Physiological Chemistry.[2]—This is a very thin book in every sense of the word. The title-page is, however, most attractive. We want clear, compact, and trustworthy "notes of demonstrations on physiological chemistry" for our students of medicine, but we shall look in vain for anything of the kind in the fifty ill-written pages before us. We fancy these "notes" were published last year in the 'Chemical News.' If so, the compiler should, at least, have taken some pains, when reprinting them, to secure consistency and freedom from typographical errors. The book, as it now stands, is really not worth the trouble of detailed criticism, but we will point out a few of the mistakes which stare us in the face as we hastily turn over the first fifteen pages.

The statement, on page 2, that vegetable products "never reach the high atomic weights found in the animal kingdom" (*sic*) ignores the existence of such a compound as vegetable albumen. We deny, moreover, that "it is generally agreed that vegetable products are ternary compounds (CHO), while animal substances are quarternary (CHNO)." Note the spelling of quaternary in passing. On pp. 2 and 3 the methods given for preparing the ash of animal matters, and for the recognition of the chief organic elements, are both inexact and obscure. Here is an example—"*Nitrogen*, by the evolution of certain odours peculiar to nitrogenous substances, and familiar as the 'burnt horn' smell." Here the author omits to state under what conditions the experiment is to be made, not even mentioning the application of heat to the substance under examination. His remarks on the distribution of the elements in animal matters (p. 4) are not satisfactory.

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[1] *Lectures on Skin Diseases, delivered at St. Vincent's Hospital.* By E. D. Mapother, M.D. With illustrations. Second edition. Dublin, 1875. Pp. 206.

[2] *Notes of Demonstrations on Physiological Chemistry.* By S. W. Moore Pp. ix and 58. London, 1874.

Mr. Moore might have omitted the paragraph on Mulder's protein (p. 5), and given us instead such a description of the method of obtaining pure albumen by dialysis as would have conveyed some notion of the process. But our author leaves out two of the conditions essential to success. His plan of preparing syntonin (p. 7) consists in "mincing a portion of flesh freed from fat, and digesting in dilute acid for some hours, filtering and neutralizing with sodium carbonate." We ask, Is fat the only substance to be removed from the flesh? May any acid be used? Will the precipitate be free from myosin? On page 14 a formula for glycerine is given, in which but two atoms of oxygen are shown instead of three, while the radicle is made a dyad instead of a triad. On the next page (15) we find the term differentiated used quite wrongly. Fats are not "differentiated by their melting points," though they may be *recognised* to some extent by observing the temperatures at which they become liquid. So, too, the spectroscope has not "enabled the chemist to differentiate apparently identical bodies" (p. 16), though it has enabled him to *distinguish* them.

It would be tedious and unprofitable to pursue any further our examination of this pamphlet. We should not have noticed it at all, but for the following three reasons:—(1) A simple, condensed, clear, and authoritative laboratory book on the subject is much needed; (2) the title of the volume before us led us to hope that this need had been at last supplied; (3) the position of the author as Demonstrator of Practical Physiology at St. George's Hospital Medical School justified us in assuming that our anticipations as to the value of the work before us would be justified.

West Riding Asylum Reports.—In previous numbers we have taken occasion to note the contents of this volume of so-called Reports on its annual appearance, and have had the gratification of generally commending them for their scientific value. Similar commendation is due to the volume last published. Some of the papers contained in it are valuable on account of original research, whilst all may be read with instruction. Amongst those of the latter class may be mentioned the lecture given by Dr. W. B. Carpenter, "On the Physiological Import of Dr. Ferrier's Investigations into the Functions of the Brain." More notable by reason of original observations are the papers by Dr. Ferrier, Dr. T. Lauder Brunton, and Dr. Herbert C. Major, treating respectively on "Pathological Illustrations of Brain Function," "Inhibition Peripheral and Central," and the "Histology of the Morbid Brain." Dr. Charles Aldridge continues his useful ophthalmoscopic observations in mental disease, and we may

[1] *The West Riding Lunatic Asylum Medical Reports.* Edited by J. Crichton Browne, M.D. Vol. iv. London, 1874.

fairly look forward to some general facts of practical value in pathology from such a series of observations sufficiently extended.

We trust that such a measure of support may be extended to Dr. Crichton Browne as will induce him to persevere with the production of this annual collection of papers on nervous and mental maladies. The title given, however, is not favorable to its success; for the article usually purveyed under the title of Asylum Reports is altogether of a different character, and commonly remarkable for its barrenness of any useful kowledge. Moreover, in no strict and proper sense can the annual volume under notice be considered a Report of the West Riding Lunatic Asylum. Its only relation to that institution is that its editor is its chief physician and superintendent, and that the asylum affords the field of observations to *some* of the authors of the papers contained within it.

Dublin Lectures on Public Health.[1]—If the public health were advanced in proportion to the amount written upon it by way of learned counsel and advice, such a healthful state of affairs would speedily follow as to render the office of sanitary medical officers a sinecure. But it is to be feared that the vast amount of sanitary teaching now expended on the public to reform its material conditions in reference to health will too far resemble, in its imperfect results, those that have attended on the preaching of truths calculated to improve mankind in its moral condition. For though it may be admitted to be more easy to move mankind in behalf of its material than of its moral interests, yet a like blindness and perversity have to be contended against in the one as in the other; and, what is more, the element of human selfishness operates with greater force in the case of the former. We must, however, be thankful for the efforts made in the interests of the public health, and must wish them "God-speed."

The contents of the volume before us have already been pretty fully placed before the profession in the weekly journals. These lectures were initiated by the Committee of Science of the "Royal Dublin Society," and carried through with the co-operation of the Council of the "Dublin Sanitary Association;" and our Irish brethren, particularly those of the capital, deserve great credit for the zeal they have shown in diffusing a knowledge of sanitary science, as exhibited, not only by instituting the course of lectures now under notice, but also by the teaching and other work carried on by Drs. Cameron, Mapother, and others.

The volume opens with "An Introductory Discourse on Sanitary Science in Ireland," by Dr. W. Stokes, whose reputation of itself was calculated to secure public attention to the subject he handled. Nine

[1] *Lectures on Public Health.* Delivered in the Lecture Hall of the Royal Dublin Society. Dublin, 1874. Pp. 203.

other lectures were successively given, by Prof. J. E. Reynolds "On the Discrimination of Unadulterated Food;" by Dr. J. W. Moore "On Meteorology in its bearing on Health and Diseases;" by Dr. James Little "On the Geographical Distribution of Disease;" by Dr. Grimshaw "On Zymotic and Preventable Disease;" by Dr. A. Hudson "On Liability to Disease;" by Dr. Robert Macdonnell "On Antiseptics and Disinfection;" by Dr. Mapother "On the Prevention of Artisans' Diseases;" by Mr. G. C. Henderson, architect, "On the Construction of Dwelling-houses with reference to their Sanitary Arrangement," and by Mr. Furlong, barrister, "On Sanitary Legislation."

From this programme it will be seen that the most important topics in sanitary science or preventive medicine were discussed, and that the volume of collected lectures may be turned to—and from the scientific standing of the lecturers, turned to with confidence—for information respecting the questions with which they deal. It is further right to add that several valuable diagrams are inserted in elucidation of the statistical and other facts brought forward; and, in fine, the volume may be recommended to all those who would acquaint themselves with the present aspect of the leading questions touching "public health."

De Renzy—Remarks on Indian Cholera Report of 1872.—The author states in page 1, "As the Report is to a great extent devoted to a refutation of certain views I have published from time to time for the last five years, I have long wished to offer some explanation in vindication of my opinions."

And as to the nature of these opinions, we find at page 22, "For many generations to come it will be impossible to trace the cause of cholera or other epidemics in this country with any sort of exhaustiveness, and all opinions regarding the origin of any particular epidemic being due to importation or not will deserve very little attention. Nor is there much hope of our being able to connect any particular epidemic with the use of tainted water by evidence of a direct specific character." And at page 1, "It appears to me that the proposition that cholera is spread by human intercourse is so fully proved that it would be superfluous to adduce any further evidence in support of it. I shall not, therefore, touch on this point any farther than to show that the facts recorded by Dr. Cunningham do not warrant the conclusion he draws from them. As regards the dissemination of cholera through water, I hope to show that the facts quoted in no way invalidate the opinion entertained on

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[1] *Remark on the Report of the Sanitary Commissioner with the Government of India on the Cholera Epidemic of* 1872 *in Northern India.* By Surgeon-Major A. C. C. De Renzy, B.A. T.C.D., Sanitary Commissioner, Punjaub. Lahore, 1874. 8vo, pp. 28.

this subject in England, and that there are other facts which strongly corroborate it."

We cannot suppose for a moment that Dr. Cuningham would have thought it necessary to occupy even a small portion of his report in criticising the views of Mr. De Renzy, merely because he entertained them, though he would certainly have been open to animadversion had he not shown the bearings of the evidence which came before him on the chief opinions now received as to the mode of diffusion of cholera, opinions entertained by many others in the profession besides Mr. De Renzy. People may differ as to the weight and value of that evidence, and Mr. De Renzy was at liberty to draw his own conclusions from it. Whether these be altogether satisfactory our readers may judge for themselves after perusing the quotations given above. We have gone over Mr. De Renzy's 'Remarks' carefully, to find whether there were any points substantiated against the positions taken up by Dr. Cuningham; we have met with a good deal of declamatory writing, but a great want of that "patient investigation of facts" which he agrees with Dr. Cuningham is the only mode of settling such questions; and we have finished the perusal with the impression that Mr. De Renzy has failed in establishing his own position, and that he has not materially affected those he attacked, while a good deal has been said which, in his cooler moments, we hope he will regret.

Shropshire Medical Tariff Scheme.[1]—This brief publication represents a praiseworthy and honest attempt to form a basis for the valuation of medical services rendered to the principal classes of society. It is issued in the name of the society, but owes its present form to the care and labour of Dr. Styrap, who has not simply sat in solitary judgment on what is right and proper, but has sought counsel and advice on all sides among those most competent to render it, and particularly in the case of the surgical tariff, with which, as a physician, he felt less competent to deal. We have considered the principles on which the tariff is constructed, and have examined the tables, and the impression we have formed is that the scale of charges recommended is one of wide application in this country and well deserving adoption by medical practitioners. We cannot here quote or discuss details, for the subject is one that scarcely falls within our province as a purely scientific journal, but we recommend our many local medical societies to procure copies, which we presume will be willingly placed at their disposal by the officers of the Shropshire Ethical Association.

Unfortunately, schemes like the present will fail, as experience

[1] *The Medico-Chirurgical Tariffs issued by the Shropshire Ethical Society.* Shrewsbury, 1874.

shows, like all other plans to make men of one mind as regards their estimate of duty and self-interest; for competition, selfishness, and greed, are disturbing forces always secretly or openly in operation, and, unless human nature gets much improved, threaten to prostrate all efforts at co-operation in the cause of virtue and mutual benefit, and in the promotion of that much-bepraised but rarely seen moral duty of fraternity.

The Toner Lectures.[1]—The "Toner Lectures," of which the present is one, were instituted at Washington by Dr. Toner, and are intended to encourage the composition of memoirs or essays relating to some novel or interesting facts in medical science. The discourse of Dr. Da Costa is devoted to the consideration of the causes producing disease of the heart, which the lecturer states to be on the increase in the United States. He divides strain and over-action of the heart into two different categories, although both may exist together, or one may follow the other; and while he denotes, by strain, some injury caused by sudden or violent effort, he defines over-action, over-exertion or over-work, as a persistent excitement and derangement due to less rapidly acting causes. As illustrations of the first, he describes several cases in which actual laceration of the valves ensued from some sudden effort, and with regard to the second, he passes in review the various occupations or amusements which tend to cause hypertrophy and valvular disease. He inclines to the belief that moderate exercise, such as rowing, does not frequently induce disease of the heart, and he has examined several persons who play at *base-ball* (the athletic national game of the United States), and he finds that it is by no means a common cause of cardiac disorders, although in those who indulge in it immoderately such a result sometimes ensues, and he gives two examples of the kind.

Chemistry in its Relations to Therapeutics.[2]—Dr. Handsel Griffiths avows himself to be an ardent disciple of the school of chemical therapeutics, by which he means, of course, the application of chemical principles to the selection and use of medicinal agents. Every one knows the difficulty, in too many instances, of reconciling the effects of drugs with their known chemical characters, and many

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[1] *The Toner Lectures. On Strain and Over Action of the Heart.* By J. M. DA COSTA, M.D., Professor of Practice of Medicine in Jefferson Medical College, Philadelphia. Pp. 28. Washington, 1874.

[2] 1. *Chemistry in its Relations to Therapeutics.* By W. HANDSEL GRIFFITHS, Ph. D., L.R.C.P.E.

2. *On the Teaching of Materia Medica and Therapeutics.* By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P.E.

3. *Monthly Report on the Progress of Therapeutics.* Edited for the 'Edinburgh Medical Journal.' Nos. III to VI. 1874-75.

practitioners are compelled to use their remedies empirically in consequence of the absence of scientific principles for their guidance. Nor are they to be blamed if they prefer to employ the method which cures their patients, or seems to cure them, rather than trifle with their sufferings in the hope, too often a vain one, of making therapeutics subservient to pure science. Nevertheless, the efforts now made to combine chemistry, both theoretical and practical, with the known laws of physiology, and to study both by the light of clinical experience, are eminently praiseworthy, and Dr. Handsel Griffiths deserves special commendation for his own efforts in this direction, which appear to lead to still greater triumphs than any yet achieved.

The Study of Life[1] is merely an exposition of the various views held by the most advanced scientific men of the present day, interspersed with some vague and somewhat peculiar views of the author himself, probably given forth to suit the author's audience and the future benighted readers of his lectures rather than being honest representations of his own beliefs. Like a great number of other popular books, this one had its origin in a course of lectures given at a debating society. Now, many extremely valuable books have been written on Life by the most talented of original and advanced observers. These works are within the reach of even the poorest of the poor, and are written in language that even the most superficially educated can understand. Hence we must recognise the fact, although unpalatable, that in this country at all events there was not the slightest need for such a book as the one in question. Literature of this class is already sadly too abundant—"Magno conatu nugas"—"Trifles with great effort." Perhaps it may be argued that this class of writers afford peculiar and limited advantages to the many in doing good according to their lights, by explaining in simple language the views of advanced thinkers, and reducing complex problems into simple and easily intelligible ones, at the least probable sacrifice of truth. But the question arises, are the efforts of many men simply confined to this end? Is not the end a distinct purpose—and that purpose to acquire knowledge at the expense of the brains and labours of their superiors? If we were to tabulate all the books written with the twofold view of notoriety and self-interest, the number would be simply immense. Of the number we would not complain for a single moment. What we do complain of, and that most vigorously, is the number of

[1] 1. *The Study of Life.* By H. MACNAUGHTON JONES, M.D., Ch.M., Fellow of the Royal Colleges of Surgeons, Ireland and Edinburgh; Senior Demonstrator of Anatomy, Queen's College, Cork, &c.

2. *The Simplicity of Life.* By RALPH RICHARDSON, M.A., M.D., Fellow of the College of Physicians, Edinburgh.

phases under which original ideas are embodied by men largely possessed of the *cacoethes scribendi* without the corresponding power of observation to write anything original, their faculty being simply one of transposing or metamorphosing words. The book-writing mania is becoming at once a nuisance and an evil. It is impossible to discover where actual truth lies, and searching for it is as arduous and as thirsty a task as searching for water in an unknown desert. There are scores of books published without a single original idea in them; there are many scores published with just one original fact, the fact being of little importance; but to pad it and increase the importance, not of the fact, but of the individual who discovered it, the public must be gulled and cozened into buying a work that contains virtually everything—with an exception—old, and nothing new.

Further, it is a notorious fact that such pseudo-authors mistake flowery language for original thought; *e.g.*, in the book before us the author speaks of "fishes rushing in countless hosts through the paths of the sea, beholding with disdain the sluggish movements of their gelatinous and crustaceous antecedents:—of a gigantic Labyrinthodon of huge crocodilian form paddling in water and roving midst ferns and huge horsetails and fir trees, with vast gaping jaws of Labyrinthian teeth, striking fright and terror into all its weaker contemporaries:—of an extraordinary fierce and remorseless ichthyosaurus, with enormous orbs and 210 teeth, pursuing with lightning speed its prey:—Of pterodactyls soaring with their huge eyes by daylight—darkening heaven with their outspreading wings, and terrifying the other inhabitants of the earth with the awakening echoes of their horrible cries," &c. &c. This style of language is certainly grandiloquent, but it is only fit for readers of novels of the Aurora Floyd type. The author's ideas are in other respects certainly above the average; still, he has much to learn before he can attain to the position of one familiar with all that has been done and is being done to bridge the gulf between man and the lower animals. Man is not separated by so vast a generality of structural peculiarities from any other animal as the author imagines. The daily record of facts from original observations and experiments show beyond the power of refutation that man never was created as an animal separate and distinct from all others, but that, like others, he has been progressively formed. He bears in every part of his body relics of a bygone condition, relics of structures found in animals lower in the scale than himself in a state of undifferentiated perfection. Man does not possess a fixed arrangement of the various tissues of his body, neither nervous nor muscular. Further, the mass of evidence points most unmistakeably towards a further goal, by the side of which man's present *integrity* and perfection are in comparison a pigmy to a giant. Any ana-

tomist who has carefully contrasted the numerous so-called irregularities met with in human subjects with those of animals lower in the scale must have common sense enough to know that these irregularities are not—as ancient anatomists once so fondly believed—"lusus naturæ." They are something far more illustrative of design and definite end. They are design and end worked out by a great and Almighty power which has manipulated by evolution, simple, or comparatively so, organisms into more complex, reflective, and self-sustaining ones, with corresponding powers to maintain them so. The osseous element, massive and grand, and enduring as it seems to us, is doomed to progressive disintegration; the motive power, or complex system of muscles, is gradually tending towards modified simplicity, while the nervous system, which seems to be both progressive and aggressive, is the dominant power before which every other kind of tissue gives way.

The question of evolution is not one to be superficially discussed. The more it is investigated the more forcibly does the truth dawn upon us that man, with everything in nature, has had a common origin. And although man is the last and best of God's great handiwork, he has been manipulated through multitudinous phases before he arrived at his present status. But the life which pervades him, whether it be a cause or a result of organization, is as much a mystery to us at the present day as it was to the ancients.

The title of the second work under review is 'The Simplicity of Life.' But we say with the champion of all modern English authors (Carlyle) that "The mysterious live embers of vital fire are not to be reduced to the degraded position of verbal simplicity." Despite the logical definitions of the meaning which certain words are meant to convey to us, and in what sense only they ought to be used, the author of 'The Simplicity of Life' has succeeded only in telling us what should be meant by what is said by such men as Huxley and Beale, as superior as scientists and observers as he would be in the art of plain speaking. To quote Carlyle again, "The speculative mystery of life grew ever more mysterious to me," and even after reading 'The Simplicity of Life' this holds good. The author has certainly advanced the stage of inquiry a step in one direction by showing that authors in future must be careful in using certain terms. But as regards what life really is, he might just as well have left his essay unwritten and unpublished for all the light that he or the authors he quotes so extensively have thrown upon this perplexing question. At the present time we do not want Dominie Sampson; there have for ages been sadly too many of this class. What we really require are earnest and simple-minded men, who are willing to undergo the necessary training to be able to study nature and her works to advantage, to be able to observe her operations and to record them in language, if not logically correct, at

least capable of being understood by others working in the same direction. The science of so-called metaphysics, equally with logic, was never a science of truth. Any question raised by the one or the other can be argued efficiently from two distinct sides, the one equally as plausible as the other. But fireside scientists, fireside metaphysicians and logicians, are babes feeding on milk in comparison with the bold adventurers into the unknown paths of biology. The author of 'Simplicity of Life' has given us a readable work. But if he had been a worker in the great field of biological science like Huxley, Beale, and others, he certainly would never have tabulated at the end of his work such a miserable *olla podrida* of oddities, neither would he have wasted his time and energies in collecting a series of assertions from every known author from Hippocrates to Huxley. The same amount of time judiciously spent in actual observation would have placed the author of the book in question in the foremost rank of original observers. It must be granted that he has given us a far more readable book than the author of 'The Study of Life.' But we must say that both have much to learn and much to do before their names will be handed down to posterity on terms similar to the names of the two or three men whom they criticise.

Transactions of the American Ophthalmological Society.[1]—We are glad to learn that the society has decided to give more permanent form to this record of its proceedings. The numbers will be paged continuously, so that four or five together will form a volume of about 700 pages. The first will consist of the reports of the several meetings from the second to the eighth, and is to be furnished by Dr. H. D. Noyes, with a profuse account of the formation of the society, and a notice of its first meeting. We should like to suggest that the price of each number be printed on the cover.

The following communications seem to us the more important; for them and for the rest—there are in the two reports before us, fifty in all—we must advise the reader to examine the 'Transactions' for himself.

H. Knapp reports 114 extractions of cataract, C. H. Agnew 118, and H. W. Williams 25, by the median flap. Iridectomy in one of Knapp's cases destroyed "an eye which before counted fingers at five feet. In this case, which looked very favorable, and would undoubtedly have done well had I made an ordinary iridectomy, I tried a new method of operating. I made an opening into the lower segment of the cornea with a lance-shaped knife, then thrust the sharp-pointed blade of a delicate pair of scissors through the iris, and cut it obliquely upward and outward; then I carried the

[1] *Transactions of the American Ophthalmological Society,* Ninth Annual Meeting, Newport, July, 1873. Tenth Annual Meeting, Newport, July, 1874. New York, 1873, 1874.

blade of the scissors back to the first point of the incision and made a second incision, nearly at a right angle with the former one, upward and inward. The angular piece of iris thus formed contracted towards its attached base, and left a beautifully clear pupil. The patient had pain during the night; purulent iritis and parophthalmitis followed from the second day. This kind of angular iridectomy tempted me by its rational appearance, yet I had tried it with the same unfortunate termination not long previously. This was in an eye where closure of the pupil had followed traumatic cataract. I made the first steps of the operation as I did in the case just related, then I endeavoured to pull the retracting triangular piece of iris out, in which attempt I did not succeed. Suppurative iritis followed, produced, I thought, by the bruising of the iris in the endeavour to draw a piece of it out. Since the avoidance of such bruising in the second case did not obviate suppuration, I made up my mind never to try again the operation."

H. Derby contributes a paper on the importance of an accurate record of all operations for cataract, in which he gives a form for a cataract-register, and suggests that:—

"1. Tables of cataract operations by a single method, intended to serve as a basis of comparison with the results of other methods, should include only cataracts that are neither the result of local injury, tangible, local, or general bodily disease.

"2. In recording vision the full fraction should be stated, the number of test-type seen, and the distance in which it is actually seen, no attempt being made to reduce the fraction to lowest terms.

"3. The test card should be artificially illuminated by a light of fixed power, placed at a stated distance from the centre of the card.

"4. The date of testing, as distinct from that of the performance of the operation, should be invariably stated. And every effort should be made to ascertain and record any variations in vision that may subsequently occur.

"5. A definite standard of success should be agreed upon; that is, that fraction should be settled which must be at least equalled by the acquired vision, in order that the result of the operation may justly be regarded as successful."

J. Green, in his remarks on the form of the corneal section, recommends the use of diagrams, three in number (front and profile view of cornea, and a part view of the iris), for securing greater precision in reporting operations and results.

Thomas R. Porley (p. 43) reports another case of scleral wound treated successfully by suture. A case of herpes zoster ophthalmicus is narrated by H. D. Noyes (p. 70), in which the one eye was lost during the attack, and the other ten months afterwards from (sympathetic) irido-choroiditis. There are two cases of herpes with loss of the eye (B. Joy Jeffries, p. 73), one where the whole side of the nose was involved without implication of the eye (O. F. Wadsworth, p. 219), and twelve cases by B. J. Jeffries (p. 221),

and (A. Mathewson, p. 228). Galvanism gave great relief to the pain in several of the latter.

H. Derby gives an account of the atropine treatment of myopia (p. 139), and appends a series of tables containing an analysis of the condition and results in sixty-seven eyes treated by him in this manner.

It is generally a very difficult problem, and one requiring much patience for its solution, to determine the refraction of the eye in certain myopic complaints—astigmatism and amphyopia. Theoretically the concave glass, spherical or spherico-cylindrical, producing the most accurate vision of distant objects, would measure the refraction, the ordinary rule, that the weakest of several otherwise equal glasses is chosen, being remembered. In these cases, however, the vision is usually very defective, even though the patient is provided with the most appropriate glasses; their use alone cannot prove the correctness of the diagnosis. The ophthalmoscope gives not unfrequently great assistance. The results attainable and the difficulties experienced are well exemplified by some cases reported in the present reports, three being of conical cornea corrected by suitable glasses (W. Thompson, p. 132) and one of extreme myopia (H. D. Noyes, p. 155). They are too long for quotation, and any abstract would fail to do them justice. A few words from the commencement of Mr. Thompson's paper will, we hope, induce some of our readers to consult the original.

"On inspection the first presented slight evidence of the conical condition, the second was more marked, whilst the third would be recognised at once as a characteristic case of conical cornea, with sight so imperfect that the patient, who had been using $\frac{1}{2\frac{1}{2}}$ spherical for several years, could only read at his far point, with his glass, viz., six inches, type of a large size; and whose correction would have been considered by me impossible by glasses but for the experience obtained from the study of the preceding ones. The high degree of acuity of vision, and the increased working power and comfort which has been gained by the correction in these cases, would encourage me to persevere in the analysis of any future ones that might present themselves before proceeding to any operation; although it would be impossible in the limits of this paper to give a true picture of the difficulties encountered in the hours of prolonged and wearisome examination into the refraction of these eyes."

The fact was observed by Wollaston and confirmed by Young that the small luminous point seen through a prism appears to be triangular. The eye cannot be accommodated so as to make the whole spectrum a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays. The amount of chromatic aberration can be measured by

glasses; that for yellow as compared with red equals about $\frac{1}{144}$, and the total amount is $\frac{1}{20}$ or $\frac{1}{18}$. Dr. J. Green points out ("On Colour-tests for Ametropia, based upon the chromatic aberration of the eye" (p. 179), that this fact may be utilised in determining the refraction of the eye, as indeed had been already attempted by Helmholtz and Pope. He says:

"We have thus a direct and delicate test for ametropia, not dependent upon the recognition of letters, and one which, unlike Steiner's experiment so admirably utilised by Thomson, does not require a large pupil for its application. In measuring myopia by this method I have succeeded best by directing the attention to the red apex of the spectrum, the weakest concave glass, through which the apex appears as a sharp red point, being the measure of the myopia for red rays. Adding to this the correction for the brightest part of the spectrum, viz., the yellow, a correction which I have found to be about $\frac{1}{144}$, we have the measure of the actual myopia in the meridian corresponding to the direction of the refracting edge of the prism. Turning the edge of the prism then through any angle, in a plane perpendicular to the axis of vision, and again measuring the ametropia in the new meridian, we detect any difference in refraction, and by thus examining one meridian after another we may work out even very complex cases of asymmetry.

"In hypermetropia, also, it has seemed to me most satisfactory to take the red end of the spectrum for observation, noting the strongest convex glass, through which the apex appears perfectly sharp, and applying the correction, minus $\frac{1}{144}$, to obtain the measure of the hypermetropia for yellow rays."

Other noticeable papers are: Cases of optic neuritis, by W. F. Norris (p. 163, with chromo-lithographs); syphilitic gumma in the ciliary body, by E. S. Loring, jun., and H. C. Eno (p. 174, with a charming etching by the latter gentleman); a new method of treating blepharospasm, by A. Mathewson (p. 207); a band of india rubber fastened to the lid and to the forehead so as to elevate the lid moderately; canthoplasty, a clinical study, by H. Althof (p. 232); and a new method of operating for strabismus, by J. F. Noyes (p. 273). We have said enough to show the considerable value of these transactions; we are glad to recommend them to the attention of English ophthalmic surgeons.—T. W.

Beigel on Diseases of Women.[1]—This first instalment of Dr. Beigel's work contains the general anatomy and pathology of the

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[1] *Krankheiten des Weiblichen Geschlechtes, vom Klinischen Pathologischen, und Therapeutischen Handpucktes, aus Dargestellt.* Von Dr. Hermann Beigel. Vol. i.

The Diseases of Women from a Clinical, Pathological, and Therapeutical Point of View. By Dr. Hermann Beigel. Vol. i.

female organs, the therapeutics of menstruation and of diseases of the ovaries. In treating these subjects the author has collected admitted facts and enumerated the various plans of treatment, instrumental and otherwise, put forward by the best writers of the day. His object would seem to have been to lay before his countrymen all the most recent improvements in gynæcology, and more particularly and fully those introduced by British practitioners, who are most copiously quoted.

The works of German, French, and American authors have also been laid under contribution. The descriptive anatomy of the female organs of generation is remarkably clear and minute, aided by well-executed plates. Following anatomical details is a chapter on the physiology of these parts. In this section the connection between the breast and uterus and other points of equal interest are touched upon, but the reader is disappointed to find that nothing new in the way of explanation of the phenomena mentioned is offered.

The changes which occur in the organs of generation during menstruation and pregnancy are next treated of, Graily Hewitt and Kundrat being quoted with respect to the condition of the mucous, and Farre as regards that of the muscular tissue. In illustration of the facts detailed, plates have been copied from the works of the above authors, and also a table compiled by Schnept exhibiting the different measurements of the uterus at different periods of life. The physiological chapter is followed by one on the medico-legal proofs of virginity. After mentioning the importance which has, in all times, attached to this subject, the author cites some of the customs pursued in regard thereto by different people. A large part of the section is devoted to a relation of the proceedings and ceremonies observed at the marriage of a chief of the Fiji Islanders, with the view of proving the virginity of the bride. This account is taken from a paper by Mr. W. T. Pritchard, published in the 'Memoirs of the Anthropological Society of London.'

In the chapter which treats of obstetrical diagnosis we have proof of the author's skill in compilation, and the engravings are abundant. Among the latter are representations of the different couches recommended for the purpose of digital or specular examination, and figures of the different kinds of instruments in use for the aid of diagnosis. In its abundance of illustrations the book at this part almost rivals a 'Maw's Catalogue,' over which, however, it has one advantage; the illustrations not only show the instruments, but also their method of employment.

The same characteristics stamp the next section, which treats of obstetrical therapeutics. Interspersed among the figures of the instruments are prescriptions for pessaries, copied from papers by

Tanner and Martin in the 'Obstetrical Transactions;' and formulæ for the administration of various drugs, by other authors.

We next come to what is called the special part (*Specieller Theil*), in which menstruation—its physiology, irregularities, and diseases—is considered.

Some space is occupied in the discussion of the ovular theory of menstruation. Dr. Barnes' Lumleian lectures on the convulsive diseases of women, and the works of other authors who have written in opposition to the ovular theory, are freely quoted.

The volume closes with an account of the diseases of the ovary. Towards the end of this section the pages again present somewhat the appearance of an illustrated catalogue.

What, indeed, must strike the reader of this first portion of Beigel's treatise is, the large amount of facts collated, the number of authors consulted, and withal an almost entire absence of original matter.

Troisier on Pulmonary Lymphangitis.[1]—The author commences his work by an account of the minute anatomy of the lymphatic vessels of the lungs. In the course of this description he mentions several experiments, by means of which he has satisfactorily proved that there exists a free communication between the lymphatics of the lungs and the cavities of the pleuræ.

With regard to the term lymphangitis, the author wishes it to be distinctly understood that he does not by its use necessarily imply that the morbid alterations he describes are, strictly speaking, of an inflammatory nature, but are due to the action of some irritating cause. The consideration of the lesion is divided into two parts—first, of that form which is dependent upon some pre-existing cancer; and, in the second place, of those less serious forms which he calls adenomatous, tuberculous, simple, and purulent.

Cancerous lymphangitis is considered under two forms, distinguished by their locality. They are called respectively superficial or pleural lymphangitis, and pleuro-pulmonary or disseminated lymphangitis. The first, as its name implies, attacks the lymphatic vessels lying on the pleural surface of the lungs. It presents the appearance of a number of whitish nodules, sending off processes in all directions. These processes are, according to Dr. Troisier, the lymphatic vessels, which present on section the appearance of canals filled with a caseous maquia. The explanation given is that the cancer is propagated along the lymphatics at the expense of their endothelium.

In the pleuro-pulmonary lymphangitis not only are the deeper lymphatic vessels of the lungs affected, but also the pulmonary,

[1] *Recherches sur Lymphangites Pulmonaires.* Par le Dr. Emile Troisier, Interne des Hôpitaux de Paris.

bronchial, and tracheal glands. The appearances are described both from a microscopical point of view and from that presented to the naked eye. The result of these observations leads the author to conclude that cancerous lymphangitis of the lung has its origin in some pre-existing cancer. This disease is seldom recognised before death, the symptoms to which it gives rise being common to it and to other forms of pulmonary and bronchial disease.

The less malignant forms of lymphangitis described by the author are adenomatous, which is secondary on a swollen and inflamed state of the lymph-glands. Tuberculous lymphangitis consists of a propagation of the tuberculous matter along the lymphatic vessels, giving rise on their walls to fresh growths of tubercle. Simple lymphangitis is seen in cases of pleurisy. If a section be made through the exudation and the subjacent pleura, the lymphatic vessels will be found gorged with material of the same nature as the exudation. Purulent lymphangitis is secondary to suppuration, either of the pleura or of the lungs. There is no doubt that, in this variety, fresh inflammatory processes are carried on in the lymphatics themselves.

The fact of the connection between the lymphatic vessels of the lungs and the pleural cavities may be of material importance in future examinations of the various pathological conditions of the lungs. The observations recorded by the author, on the subject of the propagation of cancer, may also lend further strength to the arguments put forward to prove the morbific agency of the lymphatic vessels.

Barrett on Infancy and Childhood.[1]—It is perhaps doubtful whether books on domestic medicine are worthy of notice in the pages of a scientific review: being written for the edification of the unlearned amateur they are of no intrinsic interest to the educated professional; at the same time the popular demand for such works makes it expedient that they should receive approval or the contrary at the hands of those who are qualified to judge of their merits. It is right also that the attention of the profession should be directed to the publications of this kind which issue from time to time from the general press.

Mr. Barrett states that he has written his book, firstly, "for use in emergencies;" secondly, in order that mothers who will not, or who from distance, as in the colonies, are unable to obtain the aid of "a medical man, may have that which is next best" [sed quanto intervallo?] "his written advice;" thirdly, "to give an insight into the maintenance of health and the remediability of disease."

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[1] *The Management of Infancy and Childhood in Health and Disease.* Pp. 627. By Howard Barrett, M.R.C.S., &c. London, 1875.

The first part of the book treats of the management of infancy and childhood in health—the diet, clothing, air, and exercise suitable to infantile age. The second and third parts treat of the management of children in disease—of fevers, of constitutional diseases, and disease classified according to its situation in the body; then follow surgical injuries and diseases. The book concludes with tables of diets, of weights and measures, of recipes and statistics, and has a capital index.

Out of the multitude of topics offered to the unlearned and ignorant we shall glance briefly at those likely to be of service to them, and we give our author full praise for the care with which he has elaborated those subjects.

How infants ought to be fed, nursed, and clothed, the evil consequences of improper feeding, the qualities and uses of the various artificial foods suitable to later infant life, the virtues of cleanliness, of fresh air and ventilation, are all minutely detailed, and good practical advice is given on these subjects, so greatly important to the welfare of the little ones.

Having instructed the mother as to her child in health, Mr. Barrett comes to the child in sickness.

He graphically pourtrays the expression of the face, the demeanour and gestures of the sick child, teaches the mother to distinguish the different cries of her infant—whether they are caused by hunger or pain or illness, and to recognise the sleep, the pulse, the breathing, the temperature of the skin, and the signs afforded by the mouth and tongue, the skin and flesh, and the evacuations when they vary from their normal conditions. He explains fully the benefit vaccination has conferred on the human race, nor do we think he has written a word too much, though this chapter extends over seventeen pages. The section on minor surgical injuries is practical, and the directions for the treatment of wounds, scalds, burns, stings, sprains, boils, &c., are clear and sensible. We do not, however, find any reference to fractures, and counsel as to what should be done until the surgeon arrives. Hitherto we have been well pleased with the book, but we must take strong exception to the chapter on the administration of remedies and to the second half of the book, excepting such part as we have already spoken of, and we cannot do better than quote the author himself in his own condemnation. He says:

"The second half of the book is occupied with descriptions of disease which are only intended as mere sketches. The information is not to enable the mother to undertake the medical care herself, but to arm her with salutary knowledge in case of emergency. It is undesirable and dangerous to put sharp-edged tools into the hands of those who are incapable of using them aright."

We quite agree with Mr. Barrett in these strictures, and wish he

had adhered to them in practice, and not have written the pages which follow this preface. Instead of bewildering the mother with names of diseases and numerous brief imperfect pictures Mr. Barrett might have sketched broadly the outline boundaries which separate health from disease. Had he limited the mother's pharmacopœia to castor oil, senna, sal volatile, Gregory's powder and the like, he would have been on the safer side than in recommending calomel as "a useful purgative in teething or in feverish and inflammatory complaints," or Grey powder "as a most valuable medicine in most disorders and diseases of children."

We only hope that the infants who have flourished under the healthy regimen of the first part of the book may not be done to death by the physicking described in the second, and that experimental mammas may not indulge their proclivities in trying the various recipes given in the appendix on their neighbours' or their own children.

Pathological Society's Transactions.[1]—The 'Annual Report of Council' (1873-74) now appears for the first time in the volume of the 'Transactions.' It congratulates the members upon the continued prosperity and usefulness of the Society, whilst the appended balance sheet shows that "in financial as in scientific matters the Society continues to flourish."

But what is odd in this Report, considered as an integral part of the present volume, is, that it refers to proceedings of a previous session, and to records of an important discussion on phthisis and tubercle, printed *in extenso* in a previous volume; whilst at the same time it takes no note of a like extended discussion on cancer during the session 1873-74, now reported at large in the volume before us. The inference is that this Annual Report of Council dropped out by some mischance from the volume to which it refers, and that the Report that should be attached to the newly issued volume has got mislaid.

The extent of the discussion on cancer indicates that the opinion expressed by the Council, "that future debates of the kind may be conveniently condensed, so as to interfere less with the ordinary work of the Society," was not generally concurred in by the members. The debate on cancer was as lengthy as that on phthisis and tubercle, and occupies in its report about an equal amount of space in the volume of 'Transactions.'

These records of debates on some leading pathological questions will, if not very contributory to their definite solution, be at least valuable for historical reference. Besides the record of the cancer

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[1] *Transactions of the Pathological Society of London.* Vol. xxv. London, 1874.

discussion, the volume is as usual occupied with reports on pathological objects exhibited before the Society, which of themselves render it a valuable addition to that rapidly augmenting repertorium of facts upon which the advance of scientific pathology must be based.

Infant Diet.[1]—This little book on infant diet deserves commendation. It is not made up of recipes for babies' food, with milk-and-water teachings concerning feeding; but, whilst conveying many valuable practical lessons, endeavours to instruct its readers respecting the physiology and chemistry of food and digestion and the principles which must guide them in the selection of diet. It consequently demands intelligent and thoughtful study on the part of its readers; and if it errs at all, errs on the assumption that its popular users are gifted with good intelligence and desirous of learning to deal with infants upon physicological principles, in a higher ratio than the state of society seems generally to warrant. On the other hand, the circumstance of its being in some measure the production of a doctoress will recommend it to some readers and encourage its perusal. It does not appear for how much of its contents we are indebted to Dr. A. Jacobi on the one hand, and to Madam Dr. Jacobi on the other; the contributions of the two are so blended and harmonised that the truth of the proverb that "two of a trade never agree" appears fairly contravened in this example of harmonious copartnership of medical man and medical wife.

Clinical Lectures on Various Important Diseases.[2]—We do not know if we are to accept the present small volume as an average example of the clinical teaching in the United States, for it has not been our lot to meet with treatises professedly illustrating the style and matter of such teaching in that great country. If it be an example of the current clinical teaching, it exhibits many commendable qualities, particularly those of a practical character. But when we compare these lectures with those given to students and published from time to time in our periodicals, and also as independent volumes, we observe a great contrast in regard to the minute analysis of symptoms and the discussion of pathological processes. Our English lecturers are most minute in their examinations, and most fertile in their interpretation of symptoms and of morbid appearances, and withal, as it sometimes strikes us, weak in elaborating facts which shall stand their hearers in good stead when they are called upon to deal with disease at the bedside on their own account. Such a fault,

[1] *Infant Diet.* By A. Jacobi, M.D. Revised, enlarged, and adapted to popular use by Mary F. Jacobi, M.D. New York, 1874.

[2] *Clinical Lectures on Various Important Diseases, being a Collection of the Clinical Lectures delivered in the Medical Wards of Mercy Hospital, Chicago.* By Nathan S. Davis, M.D. Edited by Frank Davis, M.D. Second edition. Philadelphia, 1874.

if it will be admitted a fault, does not present itself in Dr. Davis's clinical instruction. He examines his cases less as a pathologist than as a practitioner. He comments on the most important symptoms and reads them in the light of modern pathology, but he finds his most congenial duty to be the indication for treatment to be pursued.

The subjects selected for the lectures do not nearly range over the general field of pathology, and if the diseases discussed are, in the words of the preface, important diseases, they are so chiefly by reason of their being such as will most frequently fall to the lot of the ordinary practitioner. They are not, that is to say, important by reason of peculiar features calculated to throw light upon the less understood questions of practical medicine.

He commences with two lectures on continued fever, under which appellation he includes both typhus and typhoid fever. We find him not very clear upon the pathology of fever, and among other things he refers to disease of Peyer's glands as a feature common both in enteric fever and in typhus. "To repress the intestinal evacuations" (which, by the way, are spoken of as symptomatic of continued fever, regarded as a special morbid condition), he advocates the use of turpentine combined with opium.

As an example of his pathological and therapeutical ideas, we may quote his observations on this combination "to repress the intestinal evacuations." He considers no remedy more efficacious than the one just named; "it not only exerts a peculiar action on the mucous surface of the intestines, by which the tone or contractility of the capillaries is increased and the accumulation of blood consequently diminished, but it also increases the activity of the whole capillary vascular system. Hence it not only fulfils the local indication, but adds materially in accomplishing the third object named (viz. to sustain the functions of the nervous and vascular systems)." A modern rationalistic pathologist might object to the postulate implied in this explanation of the action of Dr. Davis's favorite medicine, and remark on the want of precision in doctrine, and of demonstration by reference to approved experiments of the peculiar action of the mixture on the intestinal surface and of the increase of tone or contractility of the capillaries.

"To devise remedies (he adds) that will relieve the extreme congestion in the lungs, and promote reabsorption of the dark blood infiltrated into the posterior and lower parts of these organs, is no easy task." However, Dr. Davis is equal to the task, and we find his suitable remedy to counteract the pulmonary congestion is a mixture of chloride of ammonium, tartar emetic, and morphia, with syrup of liquorice. He has, moreover, a second line of defence against "depression of the excito-motory centres" besides the turpentine and opium above quoted, viz., a mixture of strychnia, nitric

acid, and opium. And it will be especially gratifying to teetotallers to learn "that nearly thirty years of careful observation at the bed-side of the sick has satisfied (Dr. Davis) that strychnia is a far more reliable remedy for sustaining the nervous functions than alcohol; while the effects of the latter in diminishing the decarbonization of the blood make it positively detrimental to the already seriously embarrassed condition of the lungs."

Other lectures are devoted to the clinical history of periodical fever, of rheumatic fever, of scarlatina, of several respiratory affections and pulmonary tuberculosis, of diseases of the alimentary tract, of summer complaints of children, of dropsy and the causes of cardiac disease, of neuralgia, nervous and cerebral affections, of cerebro-spinal disease, of various cutaneous diseases of mania *à potu* and chronic disease of the brain, and, lastly, of pneumonia.

The miscellaneous character of the contents of this volume will appear from this enumeration of the subjects lectured upon. The manner and the matter are such as would be looked for from a good practical physician in passing from bed to bed in a hospital ward, and, as we commenced by saying, do not resemble the well-studied and elaborate disquisitions on disease delivered in the lecture rooms of our British hospitals under the title of clinical lectures. Each description of teaching has its advantages, and it would be well could the two be combined. But a volume of clinical lectures like that before us would be pronounced in the old world a superficial production.

On the Functional Derangements of the Liver.[1]—We owe an apology to the author and the publishers of this volume for the delay in noticing it in our pages. However, the demand for notice was, we felt, not so pressing as in the case of a book containing matter for the first time made public, inasmuch as these lectures have already been placed before the profession both in the lecture room and in the pages of our weekly contemporaries. They will have consequently won their position in public favour, and need not a commendatory notice from us. At the same time, it is both a duty and a privilege to direct the attention of our readers to the particular value of these lectures on liver derangements, knowing the large amount of instruction they contain.

The liver is a well-abused organ both by the public and by professional men, and particularly so in years now passing by. Liver derangement has long been a popular complaint, and in cases of illness where no satisfactory cause has been made out the liver has very frequently been found chargeable. In recent days liver com-

[1] *On the Functional Derangements of the Liver, being the Croonian Lectures delivered at the Royal College of Physicians, in March,* 1874. By Charles Murchison, M.D., F.R.S. London, 1874.

plaints were declining in fashion, and other organs were beginning to have their due share of blame as sources of human ills attributed to them. But after reading Dr. Murchison's able lectures we apprehend a revulsion of sentiment and to see again liver disorders in the ascendant.

For a consideration of the three functions assigned to the liver and of the various directions in which those functions may be deranged, viewed in connection with the author's classification of the derangements, opens up to our view the *fons et origo* of a vast multitude of diseases which trouble mankind. It is sufficient to notice the classification in exemplification of what we say. Under it the derangements stand as—abnormal nutrition, abnormal elimination, abnormal disintegration; derangements of the organs of digestion, of the nervous system, of the organs of circulation, of the organs of respiration, of the urinary organs; and abnormal conditions of the skin. How wide a category of diseased states is included in the area of hepatic derangement may be gathered from this classification as adopted by Dr. Murchison in discussing the subject before him.

He commences his lecture by a review of the past and present views of the functions of the liver in health, and closes by remarks on the causes of functional derangements and on their treatment, passing under notice the most celebrated liver medicines in use. With respect to mercury, he does not look upon the experiments made by Dr. Bennett and others with that drug on the lower animals as decisive of its character as a cholagogue. On the contrary, he holds that in its action it is a true cholagogue; that besides increasing "the discharge of bile from the bowel, mercury exerts a beneficial action in many functional derangements of the liver, in whatever way this is to be explained. . . . .

"It is not impossible that the good effects of mercury on the liver, and in some forms of inflammation, may be due to its property of promoting disintegration," and this view the author considers to be supported by the observation of the fact of mercury rendering "effused fibrine less cohesive, and so more readily removed by absorption than it otherwise would be." He likewise prefers mercury to podophyllin as more certain in its action and less likely to produce griping and mucous stools.

We have, in conclusion, only to repeat our high opinion of the value of this small volume of lectures, which, whilst replete with pathological facts and discussions, offers at the same time that sort of information which is most valued by those practically engaged in attendance upon the sick.

Original Communications.

I.—The Pathology of the Contracting Granular Kidney.

By T. J. Maclagan, M.D.

That form of Bright's disease which is characterised by the presence of a small, red, granular kidney, is regarded by most pathologists, not as a local disease of the kidneys, but as a constitutional ailment, whose most marked local manifestation is the production of renal mischief. This view is, no doubt, correct.

Regarding the manner in which the granular contracting kidney is produced, and the sequence of events by which the pathological change in the organ is brought about, there are three different opinions, corresponding to the three different elements of which the kidney is composed. Dr. George Johnson holds that the disease consists "primarily and essentially in disintegration and destruction of the gland-cells which line the convoluted tubes." Dr. Dickinson and others believe that the essence of the disease is hypertrophy of the inter-tubular fibrous tissue. Sir William Gull and Dr. Sutton regard this form of Bright's disease as essentially a disease of the vascular system, which does not necessarily involve the renal vessels first, and which consists in a "hyalin-fibroid" formation in the outer coat of the vessels.

The disease being a general one, we naturally turn to the constitutional derangement to see whether from a consideration of the cause of the local disease we can determine which of the renal structures is likely to be first affected. Here we are met by the difficulty that we know little or nothing regarding the constitutional cause, except that it is frequently associated with the gouty diathesis, and that workers in lead are peculiarly liable to be affected by it. There is nothing in this to debar from consideration any one of the three views which have been advanced: there is nothing in it which particularly favours any of them.

As a means of determining the question as to which of the constituent parts of the kidney is primarily affected, there remain to us, first, a consideration of the local changes which take place

in these glands; and, second, a consideration of the symptoms by which such changes are accompanied.

The disease comes on so gradually, and is so chronic both in its constitutional and local aspects, that it is impossible to fix the date of its commencement. The constitutional affection exists prior, may be long prior, to its local manifestation; and this latter has generally made considerable progress before there are produced such symptoms as render an accurate diagnosis possible, or even make the patient feel that he is ill. For this reason the malady can never be detected in its earliest stages. The renal affection is never studied before all the structures of the kidney are more or less involved. Such being the case it is difficult to see how from mere microscopic examination reliable information can be got regarding the mode of production of the changes which are noted in these glands. Such examination is most valuable in determining the changes which have already taken place; it is, indeed, the most reliable means of doing so. By the aid of the microscope we see and satisfy ourselves that there is atrophy of the tubes, with hypertrophy of the inter-tubular tissue and of the minute arteries: by its aid we can also determine the extent to which these various changes have taken place. But in endeavouring to determine in which of the structures the morbid change commenced, we find the microscope afford much less satisfactory results than the other methods of research to which we shall have recourse.

The morbid anatomy of this form of Bright's disease has of late years been the subject of much research and discussion. I do think that too much has been expected from a study of its microscopic anatomy; and that too little attention has been devoted to the careful study and interpretation of the phenomena, and changes other than microscopic, by which the malady declares its presence. Hitherto microscopic examination has led only to diversity of opinion on the part of the most accomplished observers. That structure which Drs. Dickinson, Grainger Stewart, and others regard as the one primarily involved in the renal changes, Dr. Johnson says has no real existence. The vascular changes attributed by nearly all other observers to hypertrophy of the muscular coat of the minute arteries are regarded by Sir William Gull and Dr. Sutton as the result of a change in the outer coat of the vessels.

Where such observers so widely disagree, I would not intrude an opinion founded solely on microscopic examination of the affected glands. I would rather attack the difficulty from another point, and try by a careful analysis of the symptoms and changes which accompany the development of the renal disease to advance our knowledge of the true pathology of the contracting granular kidney.

The phenomena with which we have to deal are, first, the local

changes which take place in the kidney; and, second, the symptoms and changes which result therefrom.

The local changes are (*a*) loss of gland cells, or intra-tubular atrophy; (*b*) increase of fibrous stroma, or inter-tubular hypertrophy; and (*c*) thickening of arterial coats. Now in what order do these different structures become implicated in the local disease? Can any one of them give rise to the other two? Can loss of gland-cells lead to increased growth of fibrous tissue and hypertrophy of minute arteries? Can increased growth of fibrous tissue cause loss of gland-cells, and thickening of arterioles? Can hypertrophy of minute arteries give rise to increase of interstitial fibrous tissue, and loss of glandular apparatus? Or do any two or all three changes take place simultaneously?

A careful consideration of each of these propositions, and an unprejudiced effort to explain by each the phenomena of the disease, local and general, have led me to adopt, with some modifications, Dr. Johnson's view of the mode of production of the renal changes, and to regard loss[1] of gland-cells as the primary and essential feature of the granular contracting kidney.

Holding this view, it is incumbent on us to give some explanation of the changes which we regard as secondary.

These may be considered (*a*) as they affect the kidneys, and (*b*) as they affect other organs. The former call for attention first. We shall commence with the vascular changes.

These consist in hypertrophy of the muscular coat of the minute arteries. This change in the renal arterioles, first described by Dr. George Johnson, is attributed by him to the lessened demand for blood in the kidneys, consequent on loss of their gland-cells; these vessels contracting, and ultimately becoming hypertrophied in their efforts to curtail the blood-flow to the diminished and diminishing renal tissue. That is a plausible explanation, but there appears to me to be a more satisfactory one.

Those products of retrograde tissue-metamorphosis, which it falls to the kidneys to eliminate, are not formed in these glands, at least in any quantity, but are produced in other organs whence they are conveyed in the blood to the kidneys for elimination. The formation of these excreta continuing in due amount, and the glandular surface of the kidneys diminishing in extent, it is evident that the retention of urinary excreta can only be prevented by increased rapidity of the circulation through the kidneys, and by increased activity of what remains of their glandular apparatus.

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[1] I prefer this term to that of destruction or disintegration of gland-cells. I do not think there is evidence to prove that the cells are necessarily destroyed. They unquestionably often do go through a process of granular disintegration, but such change may not commence till after they are shed, and have ceased to perform any function.

We know that the kidneys are capable of eliminating much more than the normal quantity of excreta (witness the large excess of urea which is thrown off in many inflammatory and febrile ailments, in diabetes mellitus, and in azoturia), and that if one gland be removed or destroyed, the other may do as much work as was previously performed by two. Such being the case, we cannot fail to see that loss of part of the glandular apparatus may be compensated for by increased activity of what remains sound, or comparatively so; and that even a general and equally distributed disease of both glands may, in its early stages, be insufficient to lead to such loss of function as would result in defective excretion. From what is observed in cases in which one kidney is somehow lost, it may safely be inferred that retention of urinary excreta does not take place till the normal eliminating power of the kidneys is reduced one half: that, as Dr. Bence Jones has remarked, is a large margin for safety.

In the early stage of the contracting granular kidney, the loss of gland-cells which characterizes the ailment is compensated for by sending to what remains of the glandular apparatus a larger quantity of blood for purification. The earliest result of the loss of the gland-cells of the kidneys is thus not, as Dr. Johnson holds, a diminished flow of blood to these glands, but an effort to compensate for the decrease in their eliminating surface by an increased flow of blood through them.

How is this brought about? It clearly cannot be due to increase in the *vis a fronte*, to increased attraction for the nutritive fluid in the glands themselves: the loss of renal structure which necessitates the increased flow precludes such a possibility. It must, therefore, result from increase in the *vis a tergo*, from greater force and frequency of the heart's action.

The early stage of the contracting granular kidney thus consists, first, in loss of secreting cells; and, second, in a consequent increase in the quantity of blood which flows through the kidneys in a given time; a condition which is more likely to be accompanied by dilatation than by contraction of the minute arteries.

The loss of the gland-cells being progressive, a time necessarily comes when increased rapidity of the circulation through the kidneys can no longer compensate for the decrease in their eliminating surface. Urea and other excretory products accumulate in the blood, and are conveyed to what remains of the renal gland-cells in quantities too great for their eliminating powers. This constant presence of an excess of their normal stimulant cannot fail to prove irritating to kidneys unable to respond to the call which is thus made upon them. This impression being conveyed to the renal vaso-motor centres, there results contraction of the minute arteries, which, being permanent, necessarily results in hypertrophy. There

is thus produced that thickened condition of the minute renal arteries which is noted in the advanced stage of the contracting granular kidney; a condition which, it will be seen, is an indirect result of the loss of the renal gland-cells.

What of the inter-tubular fibrous tissue? While agreeing with Dr. Johnson in regarding loss of gland-cells as the essence of the disease, I cannot homologate his opinion as to the non-existence of inter-tubular fibrous tissue. I agree with Dr. Dickinson in regarding increase of this tissue as a part of the renal changes: I disagree with him in regarding it as the essence of the disease. I believe that there is some increase of the inter-tubular tissue, but that such increase is not so extensive as Dr. Dickinson describes; that it plays an unimportant part either in the pathology or symptomatology of the disease; and that it is always secondary to the changes which have been described as taking place in the tubes and in the circulation.

Sir William Jenner has shown, and all pathologists allow the accuracy of the observation, that the continued presence in an organ of an increased quantity of blood gives rise to induration of its substance, that is, to increase of its fibrous tissue. In the kidneys such a change is frequently noted as a result of venous congestion, consequent on valvular disease of the heart, and is occasionally observed as a consequence of similar congestion resulting from the pressure of a gravid uterus. In these cases there can be no doubt that the cause which gives rise to the increased growth of fibrous tissue, which constitutes the induration, is the continued presence in the kidneys of an abnormal quantity of blood. The increase of fibrous stroma thus produced is similar to that which is noted in the contracting granular kidney; "in their minute anatomy there is no appreciable difference between the two forms" (Dickinson).

There is one very noteworthy difference, however; in the true granular contracting kidney, the organ is much reduced in size from shrinking of the tubules; whilst, in that form of induration which results from chronic valvular disease, such marked diminution is not observed.

Finding that the continued presence in the kidneys of an increased quantity of blood gives rise to an increase in their fibrous stroma similar to that which characterizes the contracting granular kidney; and finding on a careful examination of the phenomena of the latter, that there is good reason to believe that in it too there is, for a considerable period, persistent hyperæmia of the affected glands; and knowing of no other cause which is capable of producing such a result, we cannot fail here also to note the connexion between the two conditions; to associate them, as in the other case, as cause and effect, and to conclude that the increase in the inter-tubular fibrous tissue, which is found in the contracting granular kidney, results from the continued flow through these glands of an

excess of blood, an excess which is rendered requisite by the loss of part of their gland-cells.

Increase of the inter-tubular fibrous tissue we, therefore, regard as secondary to the intra-tubular changes, which thus become the primary and essential feature of the disease.

Other evidence in support of this view of the pathology of the renal changes we have in the appearance which the kidneys present to the naked eye. They are small in size, and red in colour.

Why are they small? Diminution in size is, of course, due to loss of substance. The only part of the kidney whose bulk is diminished is the renal tissue proper: the gland-cells are shed, and the tubules shrink in consequence. This shrinking of the glandular apparatus is regarded by Dr. Dickinson, and those who adopt the cirrhotic view of this form of Bright's disease, as a result of the contraction of the new growth which "encloses and compresses such parts of the gland as are in its path." This seems to me an inadequate explanation of the loss of bulk which takes place. Loss of gland-cells and falling-in of the tubules are alone quite sufficent to produce it. But if this change in the tubules be brought about by the compressing influence of a new inter-tubular growth, it is difficult to see how there could result such a marked decrease of size as is so frequently found: for, assuredly, the great and continued increase of fibrous tissue which, on Dr. Dickinson's view, takes place as the malady advances, would suffice to prevent such marked diminution in size as is noted in advanced cases. The gland substance might waste, but its place would to some extent be occupied, and the bulk of the organ to some extent maintained, by the increased growth of fibrous tissue whose presence produced, by mere physical pressure, the wasting of the tubular portion.

Why are they red? Clearly because of the presence of blood, and the absence of such changes as would produce a blanched appearance of the glands.

Here again it is difficult to see how such an appearance is compatible with the changes which Dr. Dickinson describes as characterizing the disease: for, assuredly, increased growth of fibrous tissue, which is colourless; loss of gland-cells, and obliteration of tubules, the parts to which most blood goes; and narrowing of the calibre, and thickening of the coats of the colourless vessels by which that fluid is conveyed, are changes which are all calculated to lead to bloodlessness and loss of colour, rather than to the maintenance of the normal red hue of the gland.

Neither does Dr. Johnson's view of the vascular changes consist with this appearance: as described by him these would give rise to a diminution in the quantity of blood supplied to the glands, and consequent loss of colour.

The redness of the contracting granular kidney is one of the

phenomena of the disease which must be explained. It can only be accounted for by the presence in what remains of the gland of a normal or more than normal quantity of blood. It has already been shown that the loss of gland-cells which characterizes the disease is to some extent compensated for by increased activity and vascularity of what remains: it is this increased vascularity which imparts to the contracting granular kidney the redness which forms one of its characteristics. What remains of the glandular structure has sent to it more than a normal quantity of blood for purification: it possesses, therefore, a normal, or more than normal, degree of coloration.

The effect on the renal secretion of the changes which have been noted is very marked. The condition with which we have to deal is that of a kidney which has lost its gland-cells to a greater or less extent, and through which there is a consequent and compensatory increase in the blood-flow. Now it is evident that increased rapidity of the circulation through kidneys whose tubes are unobstructed, whose gland-cells are diminished in amount, and whose vessels present no obstacle to the blood-flow, must result in increased flow of urine deficient in solids. There is, up to a certain period, no obstruction of the tubes, no hindrance to the free elimination of water, but simply loss of gland-cells, and consequent decreased power of eliminating solids. Combined with this is an increased flow of blood which, in consequence of increased force of the cardiac action, passes through the renal vessels at a higher than normal pressure. The necessary consequence of all this is an increased flow of urine of low specific gravity; and this we know is one of the earliest indications of renal mischief in this form of Bright's disease. For some time after the onset of the renal mischief the amount of urinary solids eliminated in a given time may be quite normal; but in order to effect this result there must be an increased flow of blood through the kidneys; a necessary consequence of this will be an increase in the quantity of water eliminated by these channels. In accordance with this we find that the increase in the flow of urine is less marked in the very early stages of the disease, when the diminution in the glandular apparatus and the compensatory increase in the renal circulation are slight, than it is when the renal changes are further advanced, and when the decreasing extent of eliminating surface calls for a greatly increased rapidity of the blood-flow. By-and-bye, when increased rapidity of the circulation no longer suffices to make up for the loss in the secreting cells, when many of the tubules have become shrunken and shrivelled, when urinary excreta accumulate in the blood, and when the renal arteries become contracted and thickened, the flow of urine again decreases, and dropsical symptoms may be developed, sometimes as a result, and sometimes without the occurrence, of inflammatory mischief in the kidneys.

A decided increase in the flow of urine in this form of Bright's disease marks not the earliest stage of the renal mischief, but that stage at which a much increased rapidity of the circulation is required to compensate for the loss of the renal gland-cells. Subsequent diminution in the amount of water eliminated marks the period at which the loss of the gland-cells can no longer be compensated for by increased blood-flow through the kidneys, when retention of urinary excreta becomes inevitable, and when uræmic symptoms may be looked for.

We thus find that the changes which take place in the renal secretion are likewise such as may ultimately be traced back to loss of renal gland-cells. In this connexion we find an additional argument in favour of the view which regards such loss as the essence of the disease.

Here, again, it is difficult to see how the view which regards the increase of the fibrous stroma as the primary feature of the renal mischief can adequately explain the phenomenon with which we are dealing, the occurrence of an increased flow of urine. According to this view the increased growth of fibrous tissue gives rise to such pressure on the renal tubules that their contents are more or less destroyed, and they themselves have their calibre greatly diminished. Now, it is evident that such a process of contraction and narrowing of the renal tubules could not go on for any length of time, or involve much of the renal structure, without producing a serious physical obstruction to the passage of fluid along these channels, and consequent diminished flow of urine; the very opposite of what we know takes place. According to Dr. Dickinson the physical changes which take place in the fibrous stroma are primary, and those which take place in the tubules secondary. Increased flow of urine must, according to this view, be secondary to the constriction of the tubules through which it flows; a combination and sequence of circumstances which appear to be improbable, if not impossible. According to the view which I advocate the falling-in of the sides of the tubules takes place only after their secreting cells are lost, and after they have ceased to eliminate solids, and never results from external pressure. While portions of the kidneys are undergoing this change, other parts are the seat of a compensatory hyperaction which, while it prevents for a time retention of excreta, produces an increased flow of water. Though much of this increased flow comes from tubules whose gland-cells are actively eliminating solid excreta, there can be no doubt that a considerable quantity also passes along tubules which have lost their cells, but which have not yet lost their power of excreting water. There is thus a double reason why there should be an increased flow of urine; first, because an increase flow of blood, and a corresponding increase in water elimination, are requisite to enable the sounder portions of the kidneys to throw off an extra quantity

of solid excreta; and second, because tubules which have lost their gland-cells, but are still pervious, may continue to be the channels of water elimination after they have ceased to excrete solids.

A consideration of the changes which take place both in the kidneys and in their secretion, we have thus seen, supports the view that the primary change in the contracting granular kidney is loss of renal gland-cells.

To the same source may likewise be traced back the changes which occur in other organs. We shall confine our attention to those which take place in the vascular system. Of these the most striking is hypertrophy of the heart. This is usually attributed to the difficulty which that organ experiences in driving along the impure blood. Such an explanation seems to me unsatisfactory, and that for two reasons; first, because it has not been shown that impure blood circulates with greater difficulty than pure; and second, because such hypertrophy is rarely, if ever, noted in the inflammatory and waxy forms of Bright's disease, in which the blood impurity may be as great.

Dr. Johnson regards the cardiac hypertrophy as the result of the increased force which is requisite to drive the blood through the contracted arterioles. That is a much more feasible, and, to some extent, true explanation of the increased size of the heart. But it is not the whole truth: for though in the later stages of the disease contraction of the minute arteries may increase the heart's work, it is not the initial cause of its hypertrophy. Nay, I believe that cardiac hypertrophy not only exists to some extent prior to, and independently of, the occurrence of contraction of the systemic arterioles, but that it may even have some share in initiating such contraction.

We have seen that one of the earliest results of loss of the renal gland-cells is an increased flow of blood through the kidneys, a result which is brought about by increased force and frequency of the cardiac action. This is the first step towards the production of hypertrophy of the heart. Be it noted that this increased cardiac action commences so soon as the loss of the gland cells is such as to call for increased work on the part of those which remain, and that the demand thus made on the heart continues to increase as the renal disease advances. Be it equally noted that retention of urinary excreta does not occur till a considerably later period, when increased force of the cardiac action, and increased rapidity of the circulation through the kidneys, no longer suffice to counterbalance the increased loss of renal gland-cells. The necessity for increased force of the cardiac action thus exists prior to, and independently of, the retention in the blood of urinary excreta. When such retention does take place (as with the advance of the renal mischief it inevitably must) there is brought about contraction and hypertrophy of the renal arterioles in the manner already explained. The increased

resistance to the flow of blood through the kidneys thus occasioned no doubt tends to exaggerate the already existing hypertrophy of the central organ of the circulation. That increased force is requisite to drive the blood through the kidneys at this stage of the disease has been experimentally demonstrated by Dr. Dickinson.

According to Dr. Johnson it is subsequent to the period at which such retention occurs, and as a consequence of the blood impurity which then commences, that the systemic arterioles contract with the object of excluding to some extent the impure blood. It is at a still later period, therefore, according to this view, that cardiac hypertrophy begins. Though a very plausible, I believe this to be an imperfect interpretation of the somewhat complex and complicated phenomena with which we have to deal. The true sequence of events I believe to be as follows:—There is loss of renal gland-cells, the result of some constitutional cause; there is increased activity of, and increased transmission of blood to, the cells which remain; to effect this there is increased force and frequency of the heart's action, which, being continuous, in time gives rise to hypertrophy of that organ. Now this central force, while sending an increased quantity of blood through the kidneys, must likewise send that fluid with equally increased force to all the organs of the body. The kidneys being the only organs in which such increased flow is called for, it is probable that the minute arteries of the other organs will contract to some extent so as to prevent the passage to them of such an excess of blood. The cardiac hypertrophy thus becomes the initial cause, rather than a result, of the contraction of the minute systemic arteries, other than the renal.

It is not probable, however, that the contraction thus induced can suffice altogether to prevent the passage to other organs of more blood than they really require. In this persistent hyperæmia of other organs, the liver and lungs, for example, we have a possible explanation of the changes which are frequently noted in their intimate structure in this form of Bright's disease; a change which essentially consists in increased growth of their fibrous tissue, and is probably due to the same cause (hyperæmia) as gives rise to a like increase in the fibrous stroma of the kidneys.

In the increased call upon the heart, and in this generally deranged state of the circulation, we have the explanation of the cardiac disturbance and irritability, the inability for exertion, the shortness of breath and even dyspnœa, the giddiness, the dyspeptic and other symptoms which are so prominent in this form of Bright's disease, even before there is retention of urinary excreta.

Though hypertrophy of the muscular substance of the heart and contraction of the minute systemic arteries thus exist independently of blood impurity, it is probable that the occurrence of this condition tends to exaggerate these changes in the manner described

by Dr. Johnson. The primary cause of the cardiac hypertrophy, however, is not retention in the blood of urinary excreta; but the effort which nature makes to prevent such impurity of the nutrient fluid by sending an increased quantity of blood to the kidneys for purification.

Though, perhaps, the time has not yet arrived for giving a decision on the accuracy or otherwise of the observations of Sir William Gull and Dr. Sutton, it is impossible to pass unnoticed their important contribution to the literature of the vexed question of the pathology of the contracting granular kidney.

I refer to it the more willingly as I believe that their observations and Dr. Johnson's are not so antagonistic as is usually supposed, and that they are even capable of being to some extent reconciled.

It is admitted that the granular contracting kidney is not simply a local disease of the kidneys, but a general constitutional affection. Dr. Johnson has studied the morbid anatomy of the disease from the renal side; Sir William Gull and Dr. Sutton from the constitutional. The former has described the changes which take place in the minute vessels as a consequence of the advance of the local disease. The latter have endeavoured to find the evidence of the constitutional affection, and the cause of the symptoms which exist prior to the occurrence of renal mischief, in morbid change of the minute arteries and capillaries, and have described under the name of "arterio-capillary fibrosis" a change in the outer coat of these vessels which they regard as "the primary and essential condition of the morbid state called chronic Bright's disease, with contracting kidney."

At the time that the essay of Sir William Gull and Dr. Sutton was published, I had just finished translating M. Bouchard's work on the pathology of cerebral hæmorrhage, in which is given a careful and minute description of certain changes which M. Charcot and he had observed in the minute arteries in cases of cerebral hæmorrhage, and which are described by them under the names of sclerous arteritis, chronic peri-arteritis, and arterial selerosis. On reading Sir William Gull and Dr. Sutton's paper, it at once struck me that their arterio-capillary fibrosis bore a very close resemblance to, if it was not identical with, Charcot and Bouchard's sclerous arteritis. During the last three years I have further investigated the subject, and have been led to the conclusion that the changes described as having taken place in the minute arteries by Sir William Gull and Dr. Sutton have a real existence, and that what these physicians have seen and described is the same morbid condition which MM. Charcot and Bouchard had described five years before under the name of sclerous arteritis. A comparison of the respective descriptions given by the French and English pathologists

of the changes observed by them in the minute vessels will make apparent the identity of the pathological changes observed by each. The following is a summary of Charcot and Bouchard's[1] account of what they saw:—

"This arteritis is diffuse: it extends to the entire system of small intra-cerebral vessels. Studied only in the small vessels of the brain substance, this sclerous arteritis is characterized by changes in the peri-vascular sheath, by lesions of the adventitious tunic, and also by changes in the muscular and internal coats. The principal changes take place in the most external parts; they progress from without inwards, thus justifying the name peri-arteritis. This change is observed in the arterioles of every calibre, which are situated in the substance of the brain-tissue. The lymphatic sheath may present only a striated, wavy appearance, like a bundle of subcutaneous cellular tissue. The cavity of the sheath generally presents nothing abnormal. On the principal arterioles the adventitious coat may present two different conditions. Sometimes there is simple thickening, imparting to that membrane a thickness which is occasionally equal to the calibre of the vessel. Its substance is striated longitudinally, like a bundle of fibrous tissue, and encloses fusiform corpuscles of connective tissue, arranged in the direction of the axis of the vessel. At other times, the change in the adventitious coat may consist exclusively in a multiplication of the connective nuclei, without thickening, and without a fasciculated appearance of the membrane.

"In connexion with this change in the most superficial parts of the artery, is observed a change in the muscular coat, sometimes general, at others limited to certain points. This lesion of the muscular coat consists in a change in the transverse fibres, without fatty degeneration, the result of which is thinning and separation, and at some points total disappearance of these fibres. This simple atrophy of the muscular elements does not appear to be primary, but seems to depend on the more superficial change already described. Indeed, the peri-arteritis is often seen to be limited to the sheath and adventitious coat, the muscular showing no appreciable change; it is also seen that the superficial arteritis is most marked at those points at which the muscular elements are most defective, or even altogether awanting.

"The only change in the internal tunic consists in a multiplication of the large ovoid longitudinal nuclei of that membrane. This multiplication, which may be recognised even in the true capillaries, seems to be much less marked than it is in the adventitious coat or lymphatic sheath; it may even be altogether awanting."

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[1] A study of some points in the pathology of cerebral hæmorrhage. By Ch. Bouchard, M.D., translated by T. J. Maclagan, M.D., p. 76.

Sir William Gull and Dr. Sutton say that the change which they observed "is due to a hyalin-fibroid formation in the walls of the minute arteries, and hyalin-granular change in the corresponding capillaries; that this formation occurs chiefly outside the muscular layer, but it also occurs, though to a less extent, in the tunica intima of some of the arterioles. Further, that the degree in which the affected vessels are altered, and the extent to which the morbid change is diffused over the vascular system of the different organs, vary very much in different cases. The muscular layer of the affected vessel is often atrophied in a variable degree."[1]

On comparing these two descriptions, it is evident that the same pathological change has been observed in both cases, and that the arterio-capillary fibrosis of Gull and Sutton is identical with the sclerous arteritis of Charcot and Bouchard. The description of the one would serve, indeed, for the changes seen by the other: any little difference which may appear being readily explained by the fact that the French pathologists were studying cerebral hæmorrhage, and the English pathologists chronic Bright's disease with granular kidney. Bearing in mind the frequent and intimate relationship which exists between cerebral hæmorrhage and this form of Bright's disease, it is not matter of surprise that the French and English pathologists should have found and described the same vascular changes.

I believe, then, that Sir William Gull and Dr. Sutton have given an accurate description of a real morbid change in the minute arteries and capillaries; but that they have erred in regarding this change as the primary and essential condition of chronic Bright's disease with contracting kidney. They themselves have, indeed, pointed out that such change may take place without the kidneys being at all affected.

The contracting granular kidney, and sclerous arteritis, being both diseases of adult or advanced life, and the latter being so very common, it is evident that the two must frequently co-exist.

If the former have the start and predominate, the changes in the minute arteries described by Dr. George Johnson will be more marked than those resulting from the sclerous arteritis. If the latter be well advanced before the Bright's disease set in, the changes consequent on the onset of that ailment will not be interfered with, except at points at which the muscular coat is atrophied; the two conditions will progress side by side; but those which take place as a result of the sclerous arteritis will exaggerate the tendency to cerebral symptoms (retinitis, headache, delirium, &c.), and, by rendering the vessels more brittle, will greatly increase the risk of cerebral hæmorrhage.

[1] Both Gull and Sutton, and Charcot and Bouchard are careful to point out that the changes which they describe are rare in youth, but common in advanced life.

I believe that Dr. Johnson on the one hand, and Sir William Gull and Dr. Sutton on the other, have represented with perfect accuracy changes which really take place in the minute arteries; but that the former only has dealt with the changes which necessarily occur in connexion with the granular contracting kidney. The latter observing those changes, already figured and described by Charcot and Bouchard under the name of sclerous arteritis; and observing, further, the frequent co-existence of this condition of the minute arteries, and of the granular contracting kidney, have been led (erroneously I think,) to regard arterio-capillary fibrosis as having a necessary' and causal connexion with the form of Bright's disease which we ha vebeen considering. The two conditions have, I believe, no necessary connexion, though they very frequently co-exist.

The coexistence of this change in the vessels, and of the granular contracting kidney, has a decided effect on the symptomatology of the latter, and is, I believe, one great, if not the chief, cause of the frequent occurrence of cerebral hæmorrhage in connexion with that form of chronic Bright's disease.

II.—On the Value of Fluctuation as a Sign.

By T. H. Bartleet, F.R.C.S., &c.

Fluctuation in surgical affections is a symptom so common, and is in so many cases looked upon as pathognomonic of the presence of fluid, that I have thought it might be interesting to consider its true value.

I suppose it has occurred to most surgeons to have passed a knife into a swelling, feeling assured that fluid would exude, but have felt surprise, perhaps chagrin, at the crucial test they had applied, forcing upon them the conviction of an erroneous diagnosis.

I need hardly describe what fluctuation is; we all know that the sensation is due to the incompressibility of fluid, and its consequent equal movement in all directions upon the application of pressure. The degree of movement felt depends upon many conditions, such as the limpidity of the fluid, and its quantity, the depth at which it lies, and the compactness of its covering, the thickness, and the compressibility of the sac enclosing it, and also the tension of the fluid in its sac. I need hardly illustrate this, since it is evident that a fluid becoming nearly solid or semisolid, would evidently not even intimate this sensation as clearly as a limpid fluid, and inasmuch as the depth may vary from that of the thickness of the skin to that of many inches in the human body, and the compactness of the tissues covering it from cutaneous to osseous, it is evident that this symptom fluctuation must present various degrees of distinctness, and that often only the "tactus eruditus" will enable a surgeon to ascribe to a sensation of fluctuation its true value. Various terms have been given by surgeons to describe these differences of sensation, and doughiness and elasticity are often described as varieties of fluctuation, and I think rightly so described, since, in very many cases, a sensation is perceivable by the touch which assures us of the presence of fluid, which sensation is widely different from the undulation or thud, which is the most marked kind of fluctuation, and which is so distinctly perceptible in thin-walled ovarian cysts or in many cases of ascites. I shall not enter into the general considerations of how best to detect fluctuation, viz., by the gentle tap, the gentler the better. I have often myself in large collections best felt it by baring the wrist, placing this, say, on the distended belly, and gently tapping by one finger of the same hand; the impulse is, I think, better felt in this way than by applying the two hands, as usually directed. I may mention also that when fluid is covered by œdematous superficial structures, the pressing out of the fluid in the areolar tissue will frequently enable the fluctuation to be more clearly felt.

I may also allude to the sense of fluctuation which may, and sometimes *can only*, be felt by one finger, as in post-pharyngeal abscess, or in retrouterine hæmatocele or periuterine abscess, where the presence of fluid may be frequently diagnosed with absolute certainty by a pushing or "prodding" action with one finger, but in certain cases we undoubtedly get fluid without being able to detect fluctuation as in hydroceles, where from frequent inflammation the tunica vaginalis has become much thickened, and even in very tense hydroceles, where the coverings are still thin; in some cases also of pus firmly bound down by fascia it is extremely difficult to differentiate between solid and fluid, as in deep mammary abscess. But while, on the one hand, it is often difficult to detect fluid when present, it is, on the other hand, by no means uncommon for even experienced surgeons tc come to the conclusion that fluid *is* present when it is not so. As an example, I may allude to the sense of fluctuation given by the pulpy degeneration of the synovial membrane in white swelling of the knee, and to the prognosis of cysts in breast tumours, where on section none are found to exist. But my clinical note to-day has reference to another cause of false or supposititious fluctuation. Let me relate a case.

A man was under my care for a severe injury to the left knee, and I had associated with me in the case a surgeon of the highest skill and reputation. The leg became gangrenous, and there was considerable œdema of all the tissues of the thigh. My friend insisted that there was deep-seated pus, to which opinion I demurred on strong representation, having always a respect for the opinions of others; on the following day I passed into the most prominent and fluctuating part of the swelling a fine knife, which I always carry in my case, made like a fine tenotomy knife, and which I am accustomed to say will go almost anywhere without injury. I passed this to the bone with no result. My friend still insisted that there was a bag of pus, and that I had missed the sac, so I requested him to try his hand, and he made one or two incisions, with a similar result; at last the man died and dissection showed the absence of any collection of fluid, although the symptom "fluctuation" had been most marked.

Another case, a woman with a large prominent swelling below the ensiform cartilage, which she said was hydatid cyst, and which had been tapped by a metropolitan hospital surgeon, and fluid drawn out. This patient, too, was seen by many skilful physicians and surgeons who I believe without exception came to the conclusion that there was a bag of fluid of some kind or other.

I passed an aspirator needle, and by that I mean that the aspirator was used as an exhausted needle, the stop-cock connecting the aspirator with the needle being opened, directly the needle had penetrated the skin, thus insuring that on sac existing should not be entirely passed, though, and notwithstanding this, no fluid

came. On another occasion I passed a small trochar subsequently connected with the aspirator, the trochar penetrating $1\frac{3}{4}$ inches, while on the former occasion the needle penetrated $2\frac{1}{2}$ inches, and still no fluid came.

Now, there must be some peculiar or ill-understood or ill-recognised condition which led many skilful and careful men into error, and which are constantly leading our students into similar mistakes.

I believe this false fluctuation to be generally due to the combination of two causes of error, one being muscular or glandular elasticity, and the other being muscular or glandular displacement.

I think any one who tries the experiment will be surprised at the sensation of fluctuation which can be obtained by pressing alternately, as in endeavouring to find the sense of elasticity or fluctuation of an abscess, a muscle across the direction of its fibres, say the biceps, or by similarly manipulating across the direction of the ducts, a firm and fairly large female mamma; either one of these two before-mentioned causes alone might mislead: I mean either the displacement of the gland or muscle or the elasticity of the gland or muscle: but when you get combined the elasticity and the displacement, a supposititious fluctuation is felt so like to the real as to be almost if not quite undistinguishable from it. How, then, are we to be certain, especially in these positions, where either a gland or muscle are liable to mislead us, that the fluctuation we feel is really due to fluid? By a very simple plan, which I have never known to fail, and which is not clearly enunciated to my knowledge in any of our text-books, viz. by practising the manœuvre of palpation, not only across the line of the muscular fibres or of the gland ducts, but also in a direction at right angles to this.

If the fluctuation be fluid it will be equally felt in all directions; if it be due to muscular or glandular elasticity or displacement, or both combined, it will be only felt in one direction, viz. across the muscular fibres or the gland ducts.

Let me mention one more case. I have at present under treatment in the General Hospital a young man with hip disease. There was present a barely recognisable pulsation behind the trochanter, and it was a question whether or no there was fluid. I diagnosed that there was. Now, in this position you have the fibres of the gluteus running obliquely downwards, which will give the sensation of fluctuation, while beneath these you have the gemelli pyriformis tendon and quadratus femoris running from side to side, so that it is necessary to palpate in at least three if not four directions to be quite sure that the fluctuation that is felt is not supposititious. By practising this manœuvre I was able to satisfy myself and to give a positive diagnosis of the presence of fluid, a diagnosis that was confirmed by the use of the aspirator. I would just sum up my

conclusions,—that fluctuation of the most distinct kind may be caused either by the elasticity of muscular fibres or by the displacement of muscle: by the elasticity or displacement of glandular tissue; that this only occurs in one direction, viz. across the fibres of the muscle or the general direction of the gland ducts; that palpation at right angles to this will differentiate the false and the true fluctuation, inasmuch as false fluctuation is felt only in one direction, while true fluctuation is felt equally in all directions; that where different layers of muscles take different directions, care must be taken to palpate at right angles to each layer of muscles.

Chronicle of Medical Science.

REPORT ON PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,
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Blood.

1. Mich. Laptschinsky. *Ueber das Verhalten der rothen Blutkörperchen zu einigen Tinctionsmitteln und zur Gerbsäure.* ('Wiener Akad. Sitzungsberichte,' 1874, p. 148.)
2. R. Thoma. *Der Einfluss der Concentration des Blutes und der Gewebssäfte auf die Form und Ortsveränderungen farblosen Blutkörper.* ('Virchow's Archiv,' Band lxii, 1874, p. 1.)
3. Ch. Rouget. *Migrations et Métamorphoses des Globules Blancs.* ('Archives de Physiologie Normal et Pathologique,' 1874, p. 821.)
4. R. Heidenhain. *Die Einwirkung sensibler Reizung auf den Blutdruck.* ('Pflüger's Archiv,' 1874, Band ix, p. 250.)
5. Drs. Drosdoff and Botschetschkaroff. *Die physiologische Wirkung der Waldenburgschen Apparate comprimirten Luft auf den arteriellen Blutdruck der Thiere.* ('Centralblatt f. d. Med. Wiss.,' 1875, p. 65.)

1. Laptschinsky has made experiments upon the blood-corpuscles with anilin blue, rosanilin, tannin, ammonia, and carbonic acid gas, partly in watery solution, partly in solutions of common salt of various degrees of concentration, which have led him to the following conclusions:—Every corpuscle is composed of two substances—one, the stroma of authors, is smooth, soft, extensible; the other is transparent, and can only be perceived after precipitation or swelling. It alone becomes coloured by reagents, and by its coagulation and precipitation gives rise to the most diverse microscopic images. On account of the invisibility of this last material the relation of the two substances to each other during life cannot be satisfactorily made out.

2. Thoma has studied the influence of the fluids surrounding the colourless corpuscles on the molecular processes of their amœboid movements, not only in blood withdrawn from the body, but in the living animal. If frog's blood be placed in a gas chamber, and made

richer in water by the presence of watery vapour, so that the well-known influence of water upon the red corpuscles is beginning to be visible, many (about 32 per cent.) of the colourless corpuscles exhibit lively changes of form, especially if they are adherent to the cover glass, and also movements of locomotion, whilst, if the plasma be made more concentrated by the transmission of a current of dry air, only about 12 per cent. of the cells exhibit movements, and these are of a sluggish character. In this case most of the cells undergo remarkable molecular changes; they become round, strongly refracting, their nuclei are only indistinctly visible or disappear altogether, and with high degrees of concentration the edges of the corpuscles, under high magnifying power, appear to be beset with fine and very short hair-like projections. Similar changes were seen when a drop of water was added to concentrated leucocytotic frog's blood under a cover glass, and allowed to diffuse from one side of it. The same influence of the concentration of the blood-plasma upon the white corpuscles was established in mammals, 39 per cent. of those of dogs moving actively in concentrated plasma, and 69 per cent. in diluted plasma. Similar changes were observed in the corpuscles of frogs contained within the body when the plasma was diluted by the injection of water, or was concentrated by the injection of a 3 per cent. solution of common salt, and the same occurred in the cells which had migrated into the tissues of the tongue after a wound of the tongue was irrigated with a ½ per cent. or 1½ per cent. solution of common salt.

3. Renewed investigations upon the phenomena of the circulation in the tadpole have satisfied M. Rouget that the red corpuscles in their diapedesis through the walls of the blood-vessels are completely passive. The intra-vascular pressure forces one blood-corpuscle after another through the cell-protoplasm and structureless cuticula of which the walls of the young capillaries consist. As the red corpuscles are not capable of performing spontaneous movements, they are not able to regain their pristine form, which is lost as they traverse the capillary wall. They consequently soon break down in their new position. The white corpuscles originate in the multiplication of the fixed connective-tissue corpuscles, and are carried by the lymphatics into the blood-current. By virtue of these amœboid movements they are capable of traversing the capillary wall independently of the blood-pressure. As soon as they come into contact with the red blood-corpuscles outside the vessels they invest them, and in their interior the red blood-corpuscles break up into pigment-molecules, and consequently convert the white corpuscles into pigment-cells. These last are, like the original white corpuscles, capable of spontaneous movements. They in part penetrate into the interior of the vessels, where their further destiny is unknown, and in part they form the pigmented tunicæ adventitiæ of the vessels and nerves, as well as the chromatogenous layer of the subcutaneous connective tissue; white corpuscles spring, again, from the stellate subepidermoid pigment-cells. When traumatic stimuli are applied the pigment-cells originating in white corpuscles accu-

mulate around the cicatrix and form neoplasm, the structure of which is very like the "proud flesh" that appears in the wounds of mammals.

4. Heidenhain maintains, in opposition to Cyon, that irritation of sensory nerves (when the activity of the cerebrum has been abolished in order to eliminate the influence of pain) increases the blood-pressure, and establishes the fact by new experiments that, even in chloral narcosis, the elevation of pressure only fails if the respiration be coincidentally deepened and hastened, or in those cases in which the life of the animal is in danger from too strong a dose of the narcotic, or in which much blood has been lost. In the latter case elevation of blood-pressure did not occur if the animal (not narcotized) exhibited symptoms of severe pain in consequence of the sensory irritation. If artificial respiration be performed in an animal which under the influence of chloral exhibits no elevation of blood-pressure, such elevation immediately occurs; the fall which follows the rise is partly the consequence of the quickening of the respiration, and partly of the exhaustion of the vaso-motor centre.

5. Drs. Drosdoff and Botschetschkaroff have made a series of experiments, in Professor Botkin's laboratory at St. Petersburgh, on the effects of breathing compressed air upon the blood-pressure. The method of experimenting was the introduction of a canula into the trachea, which could at will be supplied with air from a chamber in which it was compressed, or from the ordinary air. The air in the chamber was at an increased pressure of 34·2 mm., which equals $\frac{1}{22}$ of the ordinary pressure. They obtained the following results:—1. The blood-pressure fell as soon as the animal began to breathe the compressed air, and rose again directly it breathed ordinary air. 2. Coincidentally with the fall of the arterial blood-pressure the inspiratory and expiratory curves became much more marked in comparison with those observed in breathing ordinary air, being two or three times longer and higher after section of both vagi. 3. The respiration of the compressed air caused immediate depression of the blood-pressure, which fell below the normal, but rose again when the animal breathed ordinary air, and the usual changes seen after section of the vagi are observed. 4. The number of cardiac beats was but little altered by breathing the compressed air after section of the vagi. 5. The physiological effect of irritation of the peripheric extremity of the vagus during the respiration of the compressed air was weaker than on breathing ordinary air. 6. Irritation of the central extremity of the sciatic was not followed by any different effects from those usually observed when the ordinary air was breathed. On killing the animal by bleeding, whilst breathing the compressed air, no convulsions preceded death.

Circulation.

1. MM. Tarchanoff and Puelma. *Sur l'effet de l'excitation alternative des deux Pneumogastriques sur l'arrêt du Cœur.*

2. Stuart Eldridge, M.D. *Mode of Illustrating Lectures on the Circulation of the Blood.*

1. MM. Tarchanoff and Puelma, in a note read before the Society of Biology in Paris on 24th April, 1875, state that in pursuing their experiments on the arrest of the heart in mammals they have noticed a fact which appears to them to be of importance for the theory of the action of the pneumogastrics in stopping the heart's action, and to which other experimenters have not paid attention. They believed that they could maintain the heart of the dog in a state of prolonged inaction by exciting alternately the peripheric extremities of the pneumogastrics alternately. With this object in view they exposed the two vagi and took up the two peripheric extremities with ligatures. In order to observe the action of the heart, they introduced, through a hole made between the fifth and sixth ribs, a finger into the cavity of the chest, so that the movements of the heart could be directly felt. This method of observation is preferable to that made with a manometer, since this last is subject to error in consequence of the coagulation of the blood in the canula. If *one* of the vagi of a dog be excited with strong currents till its action on the heart is completely exhausted, as is manifested by the recommencement of contraction, and the opposite vagus be now immediately excited, arrest of the action does not again occur, nor indeed is there any alteration in its activity. Yet the nerve last excited has by no means been exhausted. If, however, one or two minutes be allowed to elapse before the second nerve is excited, its inhibitory action is fully exerted. This experiment shows clearly that each of the pneumogastrics calls into play the whole of the inhibitory ganglia situated in the walls of the heart, and that when once this apparatus is exhausted by the excitation of one pneumogastric it cannot be called into play by the excitation of the other. It shows also that this state of exhaustion of the inhibitory apparatus of the heart rapidly disappears on repose.

2. Dr. Eldridge, who is Surgeon-in-Chief, N. District of Japan, states that, being engaged in teaching medicine to a class of students but few of whom have a knowledge of any western tongue, and whose native language is peculiarly unfitted for conveying accurate description, even with the aid of the best interpreter, he has found object-teaching of even greater assistance than with students at home. His apparatus is ingenious, and serves the purpose well, but requires a woodcut to render it intelligible.

Absorption.

1. E. Calberta. *Ein Beitrag zur Kenntniss der Resorptionswege des Humor Aqueus.*

1. In order to settle the disputed question in regard to the nature and functions of the canal of Schlemm, Calberta introduced some fresh blood mingled with cinnabar into the anterior chamber of white rabbits, and, when the hyphæma had disappeared, hardened the eyes in Müller's solution. He found on examination that the

principal part of the cinnabar was contained in Fontana's space in the stroma and vessels of the iris, corpus ciliare, and circulus venosus, and he is therefore of opinion that Leber's view is correct, and that Schlemm's canal is not a lymphatic space.

Secretion.

1. R. Maly. *Untersuchungen über die Gallenfarbstoffe.* (' Wien. Sitzungsber.,' 1874, B. lxx, Abth. iii.)
2. Prince Tarchanoff. *Zur Kenntniss der Gallenfarbstoffbildung.* (' Pflüger's Archiv f. gesammte Physiologie,' B. ix, 1874, p. 239.)
3. H. Pink. *Zur Lehre vom Diabetes mellitus insonderheit zur Lehre von der Glycogenbildener.* ('Inaug. Dissert.,' Königsberg, 1874, and ' Centralblatt,' 1875, p. 43.)
4. G. Heidenhain. *Beitrag zum Lehre des Diabetes mellitus insonderheit zur Lehre von der Glycogenbildung in der Leber.* ('Inaug. Dissert,' Königsberg, 1874.)
5. V. Wittich. *Zur Statik des Leberglycogens.* ('Centralblatt f. d. Med. Wiss.,' 1875, p. 113.)
6. D. Friedrich Erismann. *Zur Physiologie der Wasserverdunstung von der Haut.* ('Zeitschrift für Biologie,' xi, Heft 1, p 1, 1875.)
7. MM. Musculus et De Mermé. *Sur un nouveau corps qu'on trouve dans l'urine après l'ingestion d'hydrate de chloral.* (Note read before the Academie des Sciences, April 12th, 1875.)

1. Maly has made an analysis of the biliary colouring matter biliverdin, and finds that it has the formula $C_{16}H_{18}N_2O_4$, Städeler having found it to be $C_{16}N_{20}N_2O_5$. He has also analysed a gall-stone. It was composed of almost one third of bilirubin, and contained in 100 parts—soluble biliary substances 18·09, æther extract 5·28, calcium phosphate and bilirubin combined with lime 1·41, bilirubin 28·1, ashes and loss 47·13. The residue was olive-coloured ; contained some bilirubin combined with lime, humus-like substance, and inorganic salts.

2. Pflüger remarks that it has already been demonstrated by Schiff that, if the biliary acids are injected into the intestines and undergo absorption, the liver appears to have the power of abstracting them from the blood and again excreting them in the bile. The liver, according to Tarchanoff, has the same power in regard to the biliary colouring matter. Bile excreted in a given time presents an enormous increase in the amount of colouring matter if a solution of hæmoglobin have been injected into the vessels, or even if only water have been injected (in the dog), whilst there is a relative diminution in the solid constituency, and the results of these experiments admit of a double interpretation. It may either be said that the liver forms and excretes an increased quantity of biliary colouring matter from the dissolved blood-colouring matter injected, or that this transformation takes place in the blood, and the liver only abstracts it from thence and excretes it. In favour of the latter view is the fact that

after injection of the solution of bilirubin there is an increase in the amount of biliary colouring matter excreted, and this is accomplished so rapidly and completely that no biliary colouring matter can be demonstrated either in urine taken from the bladder or from the fistula of the ureter. The same thing had already been demonstrated by Feltz and Ritter in 1870, who injected 60 grains of bilirubin into a dog, whilst Tarchanoff only injected 1·5 grains. If to this be added the fact that biliary colouring matter can be formed in the blood outside of the liver, there appears to be good reason for believing that the colouring matter of the bile is formed outside the liver and is only excreted by this organ. This affords an explanation of the yellow tint of the skin and tissues generally in icterus. Tarchanoff does not find that the injection of solution of hæmoglobin solution into the bladder causes any increase in the amount excreted by the liver, and he is therefore satisfied that the bladder is incapable of absorbing biliary colouring matters from the urine.

3. Pink first set himself to determine how long rabbits must be kept fasting to render it certain that their livers contain no glycogen, the method he adopted being to examine one part of the liver for glycogen, and to digest another with saliva and test it for sugar. He found five days of fasting requisite to remove all traces of glycogen; he then in others, after this period had passed, injected from 30 to 45 grains of purchased grape sugar into the mesenteric vein and examined the liver in the same way, and found that in the first portion of liver there was glycogen, but only traces of sugar or none at all; whilst from the second portion abundant quantities of sugar could be obtained, as the sugar in the second portion could only originate from glycogen. Schöpfer's statement was corroborated that sugar injected into the mesenteric vein is capable of being converted into glycogen. From experiments with pure sugar and with glycerine he arrived, however, at opposite results, the first portion of the liver, when *pure* grape sugar, and on one occasion when glycerine, was injected, containing no glycogen, and the second no more sugar than the first. In other experiments fine sugar and pure glycerine were injected into the stomach, where abundant formation of glycogen took place; hence Pink arrives at the conclusion that both of these substances, when taken by the mouth, are in some way altered by the stomach and made capable of conversion into glycogen.

4. Heidenhain also occupied himself with studying the effects of the injection of chemically pure grape sugar into the mesenteric vein in fasting rabbits, in order to settle the question whether sugar really undergoes conversion into glycogen. He examined one portion of the liver before or a few minutes after the injection, and a second larger portion after the lapse of from 20 to 45 minutes; the result was that, with only one exception, there was always a greater or less increase in the amount of glycogen after the injection of the pure sugar. His results are therefore opposed to those of Pink; nevertheless, the quantities of glycogen obtained were always relatively small when compared with the amount of sugar injected.

The latter amounted to from 60 to 140 grains, whilst the glycogen obtained was only from two thirds of a grain to one and a half grains. The statement of Schöpfer that the liver can convert nearly two grains of sugar into glycogen per minute is not correct. The small quantity of glycogen present Heidenhain agrees with Pink in referring to some action of the stomach or intestines on the sugar, and to test this view he tied the pylorus, and then found that after the injection of sugar but very little glycogen was formed, and there was none in the urine.

5. V. Wittich made some experiments on the amount of glycogen contained in the liver, examining first a piece removed from the living animal after previous application of a ligature to prevent bleeding, and then determining the amount of glycogen contained in the remainder after the lapse of a few minutes; he arrives at the conclusion, which is in accordance with the previous observations of Heidenhain, that the production of glycogen is either not equally distributed over the whole liver, or that the extirpation of a lobe materially diminishes the production of glycogen in the remaining portion.

6. Dr. Erismann first made some investigations upon the evaporation of water from the dead skin of man, and found that the amount of evaporation taking place from the free surface is the same whether the internal surface be moistened with water or with serum. In other experiments he found further that the amount of evaporation progressively increases with the temperature; so that whilst at the lower temperatures a few degrees make little or no difference, the same number of degrees at a higher temperature makes a considerable difference in the amount of fluid that evaporates. Care, he shows, must be taken that the skin be taken from the same or corresponding parts of the body, and if possible from the same person, for fluid evaporates from some parts much more actively than from others; as, for example, with more activity from the sole of the foot than from the chest or abdomen, notwithstanding the thickness of the skin is so much greater in the former position. It readily suggests itself that there is some connection between the number of the sweat-glands in the two parts, Krause having shown that there are 2685 sweat-glands in one square inch of the skin of the sole of the foot, and only 1136 in that of the belly. Erismann found that increased pressure produced by throwing in injection into the vessels did not augment the amount of evaporation. No difference occurred whether 1000 or 6000 litres of air was made to pass over the given portion of skin in a certain time. Little difference was observed whether the skin was covered with clothes or not. A dead body, carefully weighed after all orifices had been plugged with wax, lost in twenty-four hours 617·6 grains in weight; the temperature was on the average 63° Fahr., and the relative moisture 64 per cent. The subject was a young woman who died in childbed. Krause, perhaps operating on a larger body, found that 1900 grains were given off from the whole body when exposed to air dried with sulphuric acid. Experiments made upon

the living body showed that the most important external factor in regard to the evaporation from the surface is the relative moisture of the air; the next most important factor is temperature, the amount of evaporation increasing with its rise; and next, the ventilation, which, in opposition to its effect on the dead body, has some, though still not a considerable, influence on the living. Strong pressure in the vessels, as by drinking a large amount of hot fluid, favours materially the amount of insensible perspiration in the living body; work, even when sweating does not occur, augments the evaporation of fluid. Krause and Donders are of opinion that a good deal of the insensible perspiration takes place through the epidermis generally, though sweating is effected through the sweat-glands. Ranke and Erismann, however, hold that very little of the evaporation is effected through the epidermis, but that nearly the whole takes place through the agency of the sweat-glands.

7. MM. Musculus and De Mermé observe that the question may be asked, how and in what condition are foreign substances, introduced into the body, again ejected? and in reply it may be said that they may be divided into three groups, viz. into—

1. Substances which traverse the organism without being altered, such as creatine and acetamide, which are found intact in the urine.

2. Substances the products of the decomposition of which are found in the blood, saliva, or urine, such as leucine and glycocoll, which yield urea.

3. Substances which combine chemically with some product of the organism, and thus pass into the urine. The type of this group is benzoic acid, which, in combining with glycocoll, is eliminated in the state of benzoic acid.

Now the question arises, what becomes of chloral? According to Liebreich, this body belongs to the second group. It undergoes decomposition by the blood into formic acid and chloroform, and it is to this last that its narcotic action is due. Bouchut was the first to suggest that chloral traversed the organism without being altered. Madlle. Tomaszewic, in Hermann's laboratory at Zurich, has demonstrated the presence in the urine of a small quantity of chloral and the complete absence of chloroform. Recently Feltz and Ritter have found in the urine of dogs poisoned by chloral, chloral, sugar, and another organic substance which was, however, obtained in such small quantities that no analysis could be made of it. MM. Musculus and De Mermé have administered from 60 to 75 grains of chloral per diem to various patients, and have been able to isolate an acid having the following properties:—It crystallizes in stellæ, and under the microscope resembles tyrosine; it is very soluble in water and alcohol, less so in alcoholized ether, and still less so in pure ether; it strongly reddens litmus paper and decomposes the carbonates with effervescence; it is not displaced by acetic acid; on ebullition it reduces the alkaline solutions of copper, bismuth, and silver; it decolorizes sulphate of indigo; it rotates the plane of polarised light to the left, as does also its potash salt; it is rapidly decomposed by heat; it becomes yellowish, even

at 212° F.; on being heated with a solution of potash it becomes brown, disengaging an odour of caramel and giving up its chlorine to the potash. The authors are thus induced to think that the chloral ought to be ranked with benzoic acid in the third group, and they propose to give the acid the provisional name of urochloralic acid.

Gland.—Secretion.

1. Winkler. *Bau der Milchdrüse.* ('Jahresbericht d. Gesellsch. f. Natur und Heilk.,' in Dresden, 1874, p. 70.)

1. According to Winkler's investigation, the larger lacteal ducts in rabbits and in mice possess cubical epithelium, the smaller cylindrical. In rabbits there are some muscular fibres in the interalveolar tissue. In regard to the formation of the milk, Winkler believes that the numerous white corpuscles adhering to the alveolar walls take part in the production of the milk by undergoing fatty degeneration.

Nervous System.

1. V. Thanhoffer. *Ueber den Bau der spinal Ganglionzellen,* ('Centralblatt für die Med. Wissenschaften,' Apl., 1875, p. 305.)
2. Otto Soltmann. *Zur electrischen Reizbarkeit der Grosshirnrinde.* ('Centrallblatt, f. d. Med. Wiss.,' 1875, p. 209.)

1. V. Thanhoffer states that from repeated observation he has convinced himself that not only may two cells exist in one sheath in the spinal ganglia-cells, but that some cells in these ganglia exhibit cell-nuclei inflected at their centre, the two knob-shaped halves having a nucleolus at their distal extremities, all of which points speak strongly in favour of the division of the ganglion-cells.

2. Soltmann, repeating Hitzig's experiments on young animals, finds that the application of electrical stimuli to the cortex of the cerebrum in new-born puppies causes no muscular movements. These first make their appearance some days after birth, from the ninth to the eleventh day. The extent and form of the motor area vary, and do not correspond in young animals with those of old animals.

Generation.

1. Dr. Heintze. *Ueber den feineren Bau der Decidua.* ('Centralblatt für die Medicinische Wissenschaften,' 1875, p. 33.)
2. John Williams. *On the Structure of the Mucous Membrane of the Uterus.* (Pamphlet; reprint of paper in 'Obstetrical Journal,' 1875, pp. 28; with three plates.)

1. Dr. Heintze, in investigating the structure of the decidua, availed himself of the method of interstitial injection suggested by Ranvier, and threw in solutions of osmic acid, common salt, gelatine, and other fluids, under a constant pressure of 150 mm. He thus obtained preparations adapted for breaking up with needles, but not for section. In the former preparations fasciculi of fibres were seen,

as well as fine, delicate, isolated fibres, which became completely invisible on the addition of glycerine. Connected with and attached to these fibres and fasciculi were flat cells, the so-called decidual cells, composed of large, variously formed, and more or less granular masses of protoplasm. The endothelial cells of the tissue of the decidua were distinguishable from the endothelia of the vessels, which were also present, only by their larger size and their denser and more granular protoplasm.

The fibres were not prolongations of the decidual cells, but the cells were attached and sealed upon the fibres, as could be shown by slight pressure upon, and sliding movement of, the cover glass. In order to cause artificial œdema of the villi of the chorion, he injected into the umbilical vein a solution containing 0·5 per cent. of common salt, and then a 2 per 1000 solution of osmic acid; good sections could be made in twelve hours. The placenta had then become as hard as a board, and here and there, in consequence of the increase in volume of the villi, fissures had formed in the decidua scrotina, but in otherwise undamaged placenta the villi remained intact. Fine sections showed the tissue of the *decidua serotina* to consist of a network of fine decussating fibres, in the meshes of which, and attached to the fibres, were more or less oval cells, composed of firmly granular protoplasm and large nuclei. The structure of the decidua was thus completely analogous to the mucous membrane of the non-pregnant uterus; the only difference being that the elements of the interglandular tissue, and especially the cells, are hypertrophied, whilst the glands are transformed into cavities lined with more or less flattened cells.

2. Dr. Williams refers to the difficulties which present themselves in the way of obtaining uteri in which, at the time of death, those changes were taking place which occur normally in the organ during the menstrual flow and the intermenstrual interval. Dr. Williams has taken advantage of twelve opportunities that have presented themselves to him, in which the date of the last menstrual period could be accurately determined, to examine the changes that the mucous membrane undergoes in one entire monthly cycle. In the first case the woman died in typhoid fever whilst still menstruating. The cavity contained a few shreds of soft membrane, together with some bloody mucus. The surface was blood-stained, and attached to it were many small shreds of membrane, which gave it an uneven or flocculated appearance. On tracing the lining membrane upwards through the cervix of the uterus it was found to terminate abruptly above the internal orifice with a well-marked margin, and above this point the mucous membrane was wanting. In a second uterus, taken from a woman who had just finished menstruating, the mucous membrane was also found to be deficient near the os internum; the muscular fibre-cells were more exposed in the uterine cavity, but at the upper part they were covered with a brown layer composed of blood-corpuscles, round granular cells, fusiform cells, short rod-shaped bodies, bits of gland, and broken blood-vessels. On the addition of Liq. Potassæ the muscle-cells could be seen penetrating

the stained layer, and in many places passing through it to the surface. In a third uterus, taken from a woman of twenty, three days after cessation of menstruation, the inner surface of the uterus was pale and smooth, and there were no membranous shreds. The cavity contained but little mucus. The breach of surface at the os internum was less marked, though recognisable. The surface was studded with small white points. On section, the mucous membrane appeared as a very thin pale layer in the lower two-thirds of the body, to which it was limited. This portion of the cavity was lined by columnar epithelium, the cells of which measured $\frac{1}{3000}$th of an inch in length. The remaining third of the body, as well as the fundus, was uneven, having attached to it small brownish shreds, similar to those found in the first two uteri, but much smaller in size. They exhibited a structure similar to that presented by those in the preceding uteri, namely, fusiform and round cells, in a state of fatty degeneration, together with blood-corpuscles and ends of torn blood-vessels. The muscular fibres did not in this uterus reach the surface in its lower part, but was covered by a layer of soft tissue, composed of round and fusiform cells, short straight rods, and glands, embedded in a structureless matrix. The glands opening on the surface were not very numerous, though immediately below the surface glands were found in great abundance, apparently in different stages of development, and arranged more or less obliquely to the surface. The fifth uterus, in comparison with the third and fourth, showed considerable development of the mucous membrane, which was much thicker, and had everywhere a covering of columnar epithelium, and the glands were longer and more developed. We must refer to the original for the details of the change undergone by the uterus in the later periods, merely adding that Dr. Williams is of opinion that his observations tend to show that there is no period of uterine rest, but that the organ is ever undergoing those changes which either make it a fit receptacle for the ovum when impregnated, or which prepare to carry off the ovum when impregnation has failed. If any one stage of the monthly interval could be appropriately termed a period of uterine inactivity, that one would be, he thinks, the bleeding period, for then the mucous membrane undergoes fatty degeneration and disintegration; while this disintegration, however, is going on, the subjacent muscular wall is in a state of active proliferation for the formation of a new mucous membrane, so that in reality there is no period of uterine inactivity.

REPORT ON SURGERY.

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Gynæcological Surgery.

The treatment of cancer of the uterus by Liq. Ferri Perchlor.—The 'Brit. Med. Journ.' of February 13th, 1875, contains an article, by Dr. C. J. Gibb, of Newcastle, which gives the results of his experience in several cases of carcinomatous ulceration treated by the local application of the perchloride of iron. He says—"I have always used the strongest pharmacopœial solution undiluted, as I have only used it to secure a caustic action. At first I applied it on a piece of sponge or lint, but finally found cotton to answer best, as it sucks up any quantity that may be required, parts with it easily, and can be moulded into any form so as to fill a cavity or cover over and adhere to any growth. It has happened occasionally that I have found the cotton wool still adherent over the sore a week or more after its application, and when removed it is always a black or chocolate-coloured mass, frequently quite solid from the quantity of blood or albuminous matter absorbed in its meshes and clotted therein; indeed, one patient gravely told me she had passed a solid brown egg a few days after one of her visits. No doubt it was the hardened wool, although she declared she had cleared the vagina the day after his visit."

According to Dr. Gibb, this treatment does most good, and appears to cure even bad cases, when the disease is purely epithelial and chronic, and violent in character, and confined to the surface. The application of the iron seldom causes pain. In four or five cases the patients, on returning home, have been confined to bed for a few days, and in one of the successful cases for upwards of a fortnight, in consequence of a severe colicky pain in the region of the uterus, lower abdomen, and back. Dr. Gibb thinks that in these cases the wool has been over-saturated in the perchloride, as it has only occasionally happened that the solution has flowed from the vagina over the vulva, after the patient had left his consulting room, and those parts have been blistered and excoriated as a consequence. He, therefore, is now very careful to wash away with a syringe all discharges from the surface of the cancer, and to raise the breech of the patient to prevent any overflow of the solution over the vulva (it is much easier and more satisfactory to use Dr. P. Smith's expanding speculum, which secures an excellent view of the parts and makes a *pond* around the cervix), and after applying the perchloride to suck up with a sponge from out of the bottom of the vagina any superabundant solution which a slight pressure on the saturated wool over the sore may cause to flow out; after that, to retain the wool in place

by a loose plug of tow in the vagina; and lastly to dry and oil the vulva before the patient rises from the couch.

Emphysematous cysts of the vaginal mucous membrane.—These crepitating tumours have been observed by Prof. Braun and by Winkel in the vagina, but Prof. K. Schroeder was the first to discover emphysematous cysts in the vaginal mucous membrane; he removed two small tumours from this membrane, and by opening them under water ascertained that they contained a gas. In 'Schmidt's Jahrb. Dr. Kormann states his belief that these tumours are follicular cysts from whose serous contents gas has developed.

Pendulous tumour of the mammary areola.—Dr. McSwiney lately exhibited to the Dublin Pathological Society a tumour removed from the right breast of a single woman, æt. 50, who had ceased to menstruate six years previously. At this period she noticed a wart immediately adjoining the nipple of the right breast, which gradually increased in size and became ultimately pendulous, but as it gave her no inconvenience she did not consult any one. Seven or eight weeks before applying to Dr. McSwiney it commenced to trouble her, but was sore rather than painful, and at the very terminal point of the bulb of the tumour an abrasion of the cuticle took place. This abrasion became a sore through the friction of her dress in moving about, and from it, after a time, there exuded a muddy fluid evidently serous in character, for it stiffened the linen wetted by it. She then became alarmed, and asked for advice. The tumour was six or seven inches long and pendulous, and the bulbous extremity measured between five and six inches in circumference. The pedicle sprung from the areola, and almost from the nipple of the breast, and included the greater portion of the nipple. Its colour was normal. A considerable artery could be felt pulsating in it. The bulbous extremity had rather a remarkable appearance; it was warty, irregular, fissured, indented, and of different colours—purple in one part, grey in another, and perfectly white in the deep indentations. From the abrased portions there exuded a disagreeably-odoured fluid. Fearing that it might, if left, undergo malignant degeneration, Mr. Kane removed it at Dr. McSwiney's request. Dr. McSwiney refrained from giving any histological status to the growth, but expressed his opinion that it was innocent and consisted of an hypertrophy of one or more of the scattered glands and sebaceous follicles which abound in the areola of the female breast. In the 'Dublin Journal' of 1847 Mr. O'Ferrall gave the history of a case identical with the present one, and the drawing accompanying his description would most accurately represent the condition of the tumour Dr. McSwiney exhibited. At p. 485 of the 'Dublin Journal of Medical Science' a representative drawing is given.

Intra-peritoneal hæmatocele cured by free incision and drainage.—A woman, æt. 38, consulted Dr. John Hemans for a tumour six or seven inches in diameter in the left hypogastrium. It was roundish, not tender, movable, quite elastic, but not distinctly fluctuating, and descending into the pelvis. The uterus was normal, and could be moved to the right, but not to the left. While defecating seven

weeks previously, she was suddenly seized with a severe pain in the left hypochondriac and iliac regions, which was followed by chills. The following day she observed a tumour, which was the size of an egg and very tender, in this region. She had chills and vomiting at intervals. After ætherization, Dr. Hemans introduced the needle of an aspirator through the abdominal walls, and about seven ounces of dark reddish fluid were withdrawn. The tumour was thought to be of the same nature as a retro-uterine hæmatocele. Suppuration in the sac followed. Three days later the tumour had increased in size, and was very hard. There were pain and increase of temperature and pulse. She was ætherized again, and tapped with a common trocar through the vagina, and about two pints of dark brown, bloody, offensive fluid escaped. The sac refilled in a few days. The operation was repeated, the opening enlarged, a large elastic tube introduced, and drainage established. The sac was washed out daily with warm water. In four days the tube was removed, the discharge gradually became healthy, induration disappeared, and at the date of the report only a little inoffensive serous fluid was secreted. No subsequent trouble was experienced.—'Boston Med. and Surg. Journ.,' 4th March, 1875.

The infantile uterus.—The 'Obstetrical Journal of Great Britain and Ireland' for Feb., 1875, contains an article by Dr. Grigg on a peculiar condition of the uterus which is not unfrequently the cause of sterility. He discusses three forms, two being congenital, of which one is, and one is not, curable, and a third superinduced as a sequence of parturition at an early age, and is sometimes known under the name of super-involution of the uterus. Each of these forms is briefly described, also the various modes of treatment likely to prove available. Dr. Grigg concludes by advising a careful diagnosis, as the prognosis differs greatly in the different varieties.

Injection of tincture iodine into the cavity of uterus.—Although this paper, which is by Dr. Trask and is contained in the 'American Journal of Obstetrics, &c.,' has more special reference to post-partum intra-uterine injections, yet the gynæcological surgeon may have frequent occasion to apply this method, therefore we include it in our surgical report. After discussing the subject of intra-uterine injection of iron perchloride and stating its risks, dwelling specially on the septicæmia which not unfrequently supervenes, says—"The important question arises, are those other agents at our command, which can be substituted for the salts of iron, equally efficient in controlling hæmorrhage and free from the risks incident to these?" The perusal of the discussion at the London Obstetrical Society reminded Dr. Trask of an article which he had read many years ago in the 'North American Medico-Chirurgical Review,' vol. i, 1857, by Dr. Dupierris, of Havana, recommending the injection of tincture iodine into the uterine cavity, and giving three cases in which it was successful, and which were certainly not inferior in gravity to those reported as treated by injection of iron salts. Dr. T. says—"That recovery should have taken place in Dr. D.'s third case is very remarkable; but even in this case we see that the stimulus of

iodine was sufficient to excite the apparently extinct reflex action and secure contraction of the womb." After discussing and often combating the views of our leading gynæcologists, especially those of Dr. Barnes, who introduced the intra-uterine iron injections of Kiwisch into this country, and stating his conviction that the cases are very few indeed in which the iron acts permanently beneficially by its *coagulating* power, but rather by exciting uterine contractions, and after referring to the cases in which tincture iodine was successfully used by Drs. Emmett and G. T. Harrison and Prof. Lusk, he closes an interesting and practical paper thus:—"In recapitulation we may briefly say that we have sought to show—

"1st. That a very considerable proportion of cases in which the injection of salts of iron has apparently saved life have been those in which it accomplished this end not in virtue of its local styptic action, but because of its power to exite reflex action when cold, friction, pressure, &c., have failed.

"2nd. That when it produces coagulation of blood in the orifices of the blood-vessels, there is danger that the coagulation may follow the vessels into the substance of the uterus, producing dangerous thrombi; and that the blood already collected in the cavity of the uterus also may become converted into a hard, intractable coagulum, which the uterus cannot expel, and which may after a few days decompose and give rise to septicæmia.

"3rd. That there is evidence for believing that, as an excitor of dormant reflex action, tincture iodine may be substituted for the iron with advantage, from its efficiency as an excitor and from its antiseptic properties.

"If these points are established, the use of iron salts in a solution sufficiently strong to induce coagulation of blood in the uterine vessels should, at any rate, not be resorted to until tincture iodine has been tried and failed."

Vagino-cerviplasty in lieu of amputation of the cervix uteri.—Dr. M. A. Pallen, Lecturer on the Surgical Diseases of Women at the University of New York, contributes to the 'American Journal of Obstetrics, &c.,' the description of a new operation which will be specially interesting to gynæcological surgeons. Dr. Pallen thinks "that an amputation of an elongated intra-vaginal cervix, however great it may be, when the measurements do not exceed three inches is a mutilation, and should not be done until other procedures have failed." Further on he says—"The amputation of the cervix, as advised and performed by Huguier for hypertrophic elongation, is familiar in its details to every gynæcological surgeon, but so enthusiastic was he in its recommendation, that its dangers have in a measure been overlooked."

There are cases recorded where the peritoneal cavity was opened with the écraseur—by Marion Sims, whose case recovered after stitching the wounded surfaces; by Breslau, where the vaginal section was followed by an extrusion of the intestine; by Biefel, where death from peritonitis followed an opening into the bladder; by some Parisian surgeon (reported by Blanquinque), where death

ensued from hæmorrhage and peritonitis on the same day; by Langenbeck, where the peritoneum was also wounded; by Meadows, who described another; and by Peter, the French translator of Bennet, who mentions still another fatal one. Why such an accident takes place during écrasement is readily understood when we recollect that in all cases of hypertrophic elongation the peritoneum is dragged down with the cervix sometimes as low as the level of the sacculated bladder, almost always in the retro-uterine space, and may even pass out of the vulva, as in a specimen in St. Thomas's Museum and figured by Barnes. But to these cases the procedure of vagino-cerviplasty is not applicable; they are merely mentioned as illustrative of the dangers of amputation of the cervix by linear écrasement, to which may be added the further hazards of hæmorrhage when the conoid operation of Hugnier is made.

Vagino-cerviplasty is applicable to those cases *where the longitudinal diameter of the utero-cervical cavity does not exceed three inches, but where the intra-vaginal portion of the cervix is so long as to interfere with either locomotion, sitting, coition, menstruation, or conception;* and for the removal of which Marion Sims devised his double-flap operation, and other surgeons the galvano-cautery loop.

The next two pages are devoted to a description of the operation, and the paper concludes with the following three cases, which, are certainly very encouraging, and indicate that amputation of the cervix, except in malignant disease or hypertrophic elongation, may give place to a plastic operation that saves the woman from an unnecessary mutilation."

Case 1, operated on four years ago, was that of a woman, æt. 26, seven years married, who suffered so intensely during coitus that it had not been attempted for three years prior to the operation; she was of course sterile and somewhat hysterical. The intra-vaginal cervix measured an inch and eleven lines in the posterior and one inch and nine lines in the anterior cul-de-sac, and when the rectum was loaded with scybala the os tinca protruded from the vulva when she sat down. Forty-four months after the vagino-cerviplasty the intra-vaginal portion of the cervix was less than ten lines long, both posteriorly and anteriorly. While the sterility has not been overcome, the dysmenorrhœa is trifling, and the dyspaneuria does not exist; her marital relations, which had been unhappy, are harmonious, and the cervix is quite two inches from the vulva.

Case 2.—This case was operated on in May, 1872, The sound entered two inches and nine lines; the intra-vaginal cervix was acuminated and projected from the posterior cul-de-sac one inch and nine lines, showing that the vaginal implantation was about on a level with the os internum. The patient was thirty-one years old, and menstruated first when twenty-two years old. She suffered greatly from dysmenorrhœa, and in certain positions the elongated cervix protruded about half an inch from the vulva and irritated the clitoris. The consequences of this mechanical attrition were most deplorable and readily explained her hysteria and hyperæsthesia. There was profuse leucorrhœa and the peripheral mucous mem-

branes about the os externum constantly eroded. The vagino-cerviplasty consisted of stripping the cervix for an inch and one line anteriorly and one inch and three lines posteriorly. Seven silver sutures were passed and the patient kept in the horizontal position for nine days. On the thirteenth day the sutures were removed, and menstruation, which was less painful, supervened three days subsequently. In six months the intra-vaginal cervix was seemingly normal, with the exception of a very small os externum and her menstruations comparatively comfortable. The hysteria and hyperæsthesia ceased altogether. The patient was under observation till September, 1874, and had no recurrence of her former troubles, and was pursuing her avocations with uninterrupted satisfaction and zeal.

Case 3 was that of a lady, æt. 24, married, and of course sterile. Marital efforts had ceased for about a year, as the dyspaneuria was so great that it was followed by extreme prostration and sometimes by syncope. Leucorrhœa was abundant and constant, and there were the usual erosions on the cervix, which was slender and acuminated, with an intra-vaginal dip on its anterior surface of an inch and seven lines, and upon its posterior of one inch and eleven lines. The sound penetrated the uterine cavity to the depth of two inches and ten lines. Here was another case of implantation on a level with the isthmus. When this patient suddenly sat down the cervix impinged on the bladder, frequently giving rise to pain, always to vesical, and sometimes to rectal, tenesmus, as the inclination of the entire uterus was towards retroversion, but not actually retroverted save under the downward pressure of the superincumbent viscera, when in the sitting or squatting position. Chronic catarrh of the bladder was likewise a complication. She had been advised to submit to amputation of the cervix, and came to Mr. P— for that purpose from the State of Tennessee, but vagino-cerviplasty was performed instead, and the sutures removed on the twelfth day. Unlike the other two cases, her menstruation was usually attended with so little pain that it could hardly be called dysmenorrhœa. The subsequent vaginal hot-salt-water douches were given, and the topical applications made to the cervical erosions. The leucorrhœa ceased, but the cystitis persisted. The cervix, however, had been shortened in its dip to six lines anteriorly, and eight lines posteriorly. As she could not remain in the city for a longer period, she passed from observation after the second menstrual period subsequent to the operation. Mr. P— heard from her medical attendant in July, 1874, who stated that her dyspaneuria was quite relieved, and that the cervix was fully two inches from the vulva, and that the cystitis had almost disappeared in consequence of the removal of the irritation caused by the impinging cervix.

The treatment of vaginismus.—M. Bouchut, of the Hôpital des Enfants Malades, writing on this subject in the 'Gazette des Hôpitaux,' May, 1875, says that he has seen several young women who after marriage had at the inferior part of the vaginal orifice near the fourchette a small longitudinal fissure which was very painful to

the touch. The contact of the finger produced an acute pain just like that due to anal fissure. The same phenomenon supervened on attempting coitus and checked its accomplishment. It is not correct to say that vaginismus is only seen in non-virgins, as M. Bouchut has observed it in a virgin, affected with lymphatic leucorrhœa, who had been ordered injections, but these had to be discontinued on account of the pain which the syringe caused. In this case a slight fissure of the hymen was the cause of the pain.

M. B— does not think that forcible dilatation of the vagina, as in a similar affection of the rectum, is the best mode of treatment, and recommends the following plan before having recourse to operative measures. He says—"In many cases I have cured patients without operation and by the most simple means, such that every medical man may employ every day. These consist in the use of vaginal suppositories containing cacao-butter five grammes, extract of rhatany three grammes, and of baths of bran-water. One suppository should be introduced night and morning, then every day for an hour the patient should take a bath of bran-water."

In this way M. B— cured several cases of vaginismus without having recourse to an operation as disagreeable to the women as to their husbands.

Hydrocele in a female.—Dr. G. A. Baxter ('Southern Med. Record,' Feb., 1875) narrates the case of a woman, æt. 32, who two years previously, while lifting a heavy bucket of coal, felt a sharp pain in the inguinal region, which passed away in a few minutes. Some days after she experienced pain in the same region, extending into the labia majora, and accompanied by swelling. These symptoms continued, when, on lying down one day, the swelling and pain suddenly and almost entirely disappeared.

The tumour remained in this state for nearly two years, when, having fallen over a chair, the labia majora became bruised, and the tumour began to increase, without giving any pain. Dr. Baxter found a tumour as large as an ordinary egg, whose apex pointed to the external inguinal ring, and whose base was large, and caused bulging of the upper portion of the labia majora. The swelling was first taken to be a hernia or an abscess, and under the latter supposition it was opened, when serum instead of pus was evacuated. She rapidly recovered.

The treatment of uterine fibroid by ergotin.—The 'Clinic' of January 23rd, and the 'Philadelphia Med. Times' of May 1st, 1875, contain the account of a case of submucous fibroid, recently under Dr. Bartholow's care. An aqueous solution of ergotin, in one-grain doses, was hypodermically injected night and morning. The following afternoon the patient began to feel expulsive pains, which increased continually in severity. After a fortnight the injections were used only once a day. A week later the pains had diminished in severity, and on examination the neck of the uterus was found very much dilated and plugged with a tumour, which was then easily removed with the écraseur. Full recovery ensued, and the patient has not made any complaint since the growth was removed.

In connection with this subject we could draw the attention of our readers to an article by Dr. Hildebrandt, "On the Treatment of Uterine Myofibroma of the Uterus by the Hypodermic Injection of Ergotin," in the 'American Journal of Obstetrics and Diseases of Women and Children' of Feb., 1875, which contains a full account of the preparation, mode of application, advantages, &c., of this mode of medication in uterine fibro-neoplasms.

Extraction of a calculus from the bladder through a vesico-vaginal fistula.—Mr. Teevan, of the West London Hospital, has lately had under care a woman, æt. 66, the subject of a vesical calculus. There was a fistula in the roof of the vagina, which admitted the tip of the finger (index ?), and a stone, apparently of small size, was felt with the sound, lying close to the fistulous opening.

On September 1st, the patient being ætherized, Mr. Teevan introduced a pair of narrow-bladed forceps into the bladder through the fistula, seized the stone, which was lying just above the ovoid opening, and extracted it. The calculus was phosphatic, and measured $1\frac{3}{8}$ inch in length and $2\frac{3}{4}$ inches in circumference. Though incision was made but few drops of blood escaped from the distended edges of the fistula. The bladder was then examined with the finger, and found to contain a large quantity of *débris*, which was mortar-like, and was removed with a scoop. The viscus was washed out, but no attempt was made to close the fistula, as the parts were in too unhealthy and inflamed a condition to admit of any operation. The patient was much relieved, but still complained of "soreness," owing to the urine dribbling away, and was discharged at her own request on September 22nd.

Soon after her return home she again began to pass "grit," and complained of pain. Shortly after her feet and legs became dropsical, and she died on Dec. 6th, rather more than three months after the stone was extracted.

Mr. Teevan thought that the fistula was due to the ulceration accompanying chronic cystitis, or to the result of the employment of a metal catheter, which had been used for some time by an unprofessional person.

On serous ovarian cysts.—Dr. Panas read a paper on this subject at a recent meeting of the Académie de Médecine, and drew the following conclusions, which are contained in 'Le Mouvement Médical':—

1. That among the cysts called ovarian there is a class of unilocular cysts containing a special fluid, the treatment of which is as simple as it is certain in its results.

2. The characters of the cystic fluid are, complete absence of viscidity, perfect diaphaneity (with occasional exceptions), poverty in protenious material (modified albumen), and its relative richness in alkaline salts, principally chloride of sodium. Slightly or not at all precipitated by heat and nitric acid, the liquid in question is precipitated by alcohol. In this respect this fluid bears a certain analogy to that found in the spermatic cysts of men, as we may be convinced by comparison of the two fluids.

3. We are at present ignorant as to whether the point of origin of these cysts is actually in the ovary, or is not rather in the parovarium (body of Rosenmüller).

4. The treatment of these cysts is still more simple than that suggested by Boinet, who proposed puncture by means of a trocar, followed by injection of iodine. A simple puncture by the trocar is sufficient to bring about a cure, by removing the fluid completely or even partially.

5. By this process not only is all danger avoided, but even the slightest pain to the patient. In a word, the treatment of these cysts is easier than that of simple or spermatic hydrocele in men, which requires, almost always, the subsequent employment of caustic or strongly irritating injections.

We need scarcely add that, both for patient and surgeon, Dr. Panas's paper is a most important one. We should be very glad to know if British or American experience corroborates the views set forth in it. At present we are under the impression that our experience of tapping, even in such cases, is not by any means so favorable, as regards cure, as that which Dr. Panas seems to have experienced.

Dermoid cyst of the ovary.—M. Terrier, in 'Bulletin Gén. de Thérap.,' 15th March, 1875, narrates the case of a patient who came under his care for a large tumour occupying the right iliac fossa, and presenting all the characters of an ovarian cyst.

An exploratory puncture revealed the existence, in this tumour, of epithelial cells, of hair, and of a thin greyish-white liquid. An operation was decided on, and was executed without difficulty, the results being favorable. The temperature never exceeded 100·5° F. Without appreciable cause the urine became retained at the end of five days. (Retention of urine is not uncommon after ovarian abdominal section, and it is the best practice to draw off the urine, and spare the patient the discomfort and straining which often accompany voluntary micturition in such cases.—*Rep.*)

Microscopic examination of the sac showed that it presented, on its internal aspect, the appearance of skin. In fact, hair, fatty granules, sudoriferous glands, &c., were found.

Stoltz's operation for cystocele.—We recently saw Dr. Heywood Smith operate, at the Hospital for Women, on a case of cystocele; and at the suggestion of Professor Stoltz, lately of Strasburg, and now of Nancy, performed his (Professor Stoltz's) operation for the relief of this deformity. The method consists in removing a circular piece of mucous membrane about the size of a five-shilling piece, and, when bleeding is checked, in passing a silk suture around the bared surface, about a quarter of an inch from its margin, in the same manner as is done in making an ordinary calico bag. On drawing the two ends together the edges of the wound are approximated, and the calibre of the vagina much diminished in length and circumference.

The after-treatment resembles that of other plastic vaginal operations. The result was very satisfactory, the prolapse of the uterus

being overcome, as its cervix was retained in a pouch above the narrowed vaginal surface. The woman was middle-aged.

We draw attention to this operation, not only because of its success, but also because we have not been able to find any account of in it any works on gynæcological surgery that we have read.

Four cases of echinococci in the female pelvis.—Drs. Freund, of Breslau, and Chadwick, of Boston ('American Journal of Obstetrics, &c.'), after referring to the extreme rarity of this disease, and showing that the cases previously supposed to be of this nature were in all probability hydatid moles, refer to the case of Dr. Graily Hewitt as "the first perfectly authentic case on record in which true hydatids have been found in the uterus," and say that their first case belongs to the same category; it is as follows:

Case 1.—*Echinococci in the posterior wall of the uterus; incision with evacuation of hydatid vesicles; new colonies in the pelvic cellular tissue; spontaneous perforation into the bladder and rectum.*—M. S—, æt. 57. Menstruation began in twentieth year, had always been scanty; was married at twenty-five, and been sterile. Menses ceased several months ago. Hardness in lower part of abdomen. Has suffered much for past six years from difficulty of micturition and defecation, as well as a sense of weight in the abdomen. For several weeks the retention of fæces and urine has been nearly complete. Only produces dejections by means of large enemata passed very high up through a long tube.

On examination the bladder was found distended so as to rise three inches above the pelvis, and was displaced somewhat to the right. To the left of the bladder, in the inguinal region, was a smooth elastic tumour. The vagina was reduced to two thirds of its normal length by an elastic tumour that completely filled the small pelvis. Upon passing the catheter to evacuate the bladder, the uretha was found greatly elongated, as happens with retroflexion of the gravid uterus. A great quantity of thick urine was drawn off. The tumour was clearly recognised as belonging to the posterior wall of the womb, for it could be traced into the posterior lip of the os, which was completely drawn out. The external os appeared as a narrow transverse cleft, bounded posteriorly by the tumour, and anteriorly by the very attenuated anterior lip. By the bimanual examination the uterus was found to correspond in size with the pregnant organ at the third month. The sound entered five inches, and showed that the anterior wall was extremely thin throughout its whole extent. Sim's speculum brought only the surface of the tumour into view, the os being pressed forward against the symphysis pubis. Upon these data a cystic tumour of the posterior wall of the womb was diagnosed, and it was thought to be a fibro-myoma with a secondary cyst.

In order to relieve retention of urine and fæces, as well as to aid the diagnosis, a trocar was introduced into the tumour very near the os. As a clear watery fluid gushed from the canula, the opening was at once enlarged with a bistoury, and a great amount of uid escaped, containing numerous vesicles varying in size from a

pea to a cherry. The posterior wall did not collapse as much as might have been anticipated from the amount of fluid evacuated; it felt thick and rough. The authors emphasize this point as suggesting a likelihood of the parent-vesicle being located in the uterine wall. The fluid was limpid, contained no sediment, and only traces of albumen. A more exact clinical examination was superfluous, because the microscope told the tale; with it structureless stratified membranes, scolices, and separate echinococci-hooks were seen.

During the week after the operation a thick greenish fluid containing shreds and flakes of membrane escaped from the incision. She said that immediately after getting home she was taken with high fever and symptoms of gastro-enteritis, accompanied by a profuse discharge of very offensive fluid from the vagina. General health good, but micturition and defecation were few (*sic*). The nodular and enlarged uterus was moderately anteverted; the posterior wall still much thicker than the anterior, but in the right side of the anterior wall were felt irregularities and round hard nodules. Precisely similar nodules were felt behind and to the right of the womb; the bladder was firmly adherent to this mass, whereas on the left side it was freely movable with the catheter and distensible. With the speculum the anterior lip was seen to have become much thicker than before, and presented many degenerated follicles. The posterior lip was rough, and of a grey colour.

In April, 1871, she again appeared with fresh complaints. She had a constant desire to urinate, and when successful she had much strangury. She exhibited many vesicles which had passed with her urine. A large catheter brought away a little thick urine filled with vesicles and shreds of membrane. She said that similar membranes and pus had often been discharged from the vagina after severe tenesmus. A firm infiltration of the cellular tissue to the right and behind the womb was made out; in this the cervix was embedded. The discharge through the bladder ceased about the middle of May. In October, 1872, she brought vesicles which she had passed through the rectum. She had had several profuse hæmorrhages from the vagina. The womb was more firmly held than before by the parametric infiltration, which then half enveloped the rectum with an unyielding arch, yet the mucous membrane of the rectum was only attached to the underlying tissues at one point; no perforation could be discovered. Through the speculum the vaginal portion of was seen to be very broad; the anterior lip differed completely from the posterior; it surrounded the latter like a half-moon, and was only one third as thick. Its appearance was peculiar. The surface was smooth, the mucous membrane seemingly intact; its external border was of carmine-red colour, the rest of brick-red with brighter radiating stripes; in the spaces between these latter yellow points, varying in size from a millet-seed to a pea, were scattered. The posterior lip looked like a complete vaginal portion; it was greyish, rough, and irregular. The yellow points in the anterior lip proved not to be vesicles, but only ovula Nabothi.

The chief interest of this case rests upon the unquestionable

presence of echinococci in the parenchyma of the uterus. That this site of the vesicles was primary is more than can be asserted, for, as the same time that they were found in the womb, they were likewise discovered in the surrounding tissues. They may have been first developed in the uterine walls, or have penetrated into them from the cellular tissue, or even from the peritoneal cavity. In the latter alternative, the case would be analogous to those in which the same entozoon has been known to pass from the pericardium into the substance of the heart; and according to the best pathologists it is never developed primarily in the last-named organ. The case is, at any rate, a new proof that echinococci may exist in the parenchyma of the uterus.

Case 2.—*Echinococci in the liver, spleen, great omentum, and pelvic cellular tissue.*—Miss J—, æt. 22, began to menstruate when fifteen, and has always been regular. Has been in poor health for three years, and her abdomen has been gradually swelling in its upper and middle parts. Catamenia have lately been more scanty, and there is frequent desire to micturate. Is much emaciated, left thorax is somewhat distended, the heart pushed upward and to the right, with its apex in second intercostal space. Beneath the heart is the spleen, much enlarged, and extending downwards to the edge of the ribs. The liver extends from the fourth rib to two inches below the thorax. Uterus pushed down towards pubes. A part of the stomach can be made out by percussion projecting beneath the spleen. The ascending, transverse, and descending colon can be distinctly traced around a tumour, whose surface is broken up by smooth prominences. This reaches to the symphysis pubis, and gives the most perfect hydatid tremor on percussion. Vagina much compressed, and uterus held firmly against pubes by an elastic tumour, which projects from the right side of small pelvis towards the middle. Rectum pushed over to right and somewhat constricted, yet defecation not impeded. Puncture of tumour per vaginam revealed all the characteristics of echinococci; after its evacuation small round tumours were still to be felt behind the uterus. The cyst was evacuated, and the tumours remaining in the pelvis are unmistakably situated in its extra-peritoneal cellular tissue. It was assumed that the hydatids composing the abdominal tumour were in the omentum, and also extra-peritoneal. In these particulars the case resembles one reported by Leudat (' Gaz. Méd de Paris,' No. 27, 1856), where the diagnosis was corroborated by the autopsy.

Case 3.—*Echinococci of liver, spleen, great omentum, and probably of right ovary.*—Mrs. R—, æt. 25, first menstruated at fifteen, and has had one child. The right side of the abdomen has been enlarging for six months. Menstruation is regular. During last six weeks the tumour has been growing rapidly, and there are severe abdominal pains, fever, great thirst, and prostration. The heart is displaced as in the previous case, but the spleen is even larger and projects two inches below the edge of the ribs. The liver extends from the third rib to three inches below the margin of the ribs; the left lobe is chiefly enlarged, and forms a very prominent tumour, which gives

the hydatid tremor. This tumour is separated from the symphysis pubis by convolutions of the intestines. The uterus is easily grasped bi-manually and is anteverted. Obliquely behind it is a small cystic tumour resembling in every respect an ovarian tumour. To relieve the dyspnœa the cyst in the left lobe of the liver was punctured three times at intervals of three weeks. It discharged the usual contents of an hydatid cyst; the last time of tapping the fluid was purulent and the cavity did not refill. The omental tumour was similarly treated, but was not completely emptied. The patient felt so well that she declined any further treatment.

Case 4.—*Echinococci, of hepatic origin, in Douglas's pouch.*—Mrs. R—, æt. 55, had a tumour on right side of abdomen, but knew nothing of its mode of growth. Abdominal organs seemed healthy, except the genitals. The senile uterus was pushed forwards and to the left side by a tumour as large as a child's head at birth; it was rough, hard, and not fluctuating, and seemed to be adherent to the posterior wall of the uterus, and not to be connected with any other abdominal organ. Her sufferings arose from repeated attacks of circumscribed peritonitis in the region of the tumour. She died of some intercurrent affection and a firm tumour was found at the spot mentioned, which was adherent to the uterus, and at first appeared to have no connection with the other organs; but a careful search discovered a band a foot long and as fine as a thread, running to the right lobe of the liver. The tumour consisted of a dead echinococcus cyst with an immense number of secondary vesicles. The connecting band and the deep yellow colour indicated the liver as the original site of the cyst. It had developed in the liver, protruded and become pediculated, and hanging down into the pelvis had formed adhesions there. In this connection we may refer to a recent paper by Dr S. Spiaggia in the *Gazette Clinica dello Spedale Civico di Palermo*, January, 1875, on "Oligæmia from Metrorrhagia and Acephalocysts of the Uterus," &c.

Virchow says that echinococci develop in the lymphatic vessels; each cavity, containing a parent cyst, has two walls, one belonging to the organ and one to the animal. The membranes of the cyst, when discharged, always roll up inside out, raising a doubt among the ignorant as to whether the animals do not grow on the outside. If there is any roughness on one side of the membrane, it is safe to conclude it is the inner surface. These irregularities are not as a rule young echinococci, but only pathological appearances common to old age. On section the membrane is seen to consist of many strata, arranged with the greatest possible regularity. Each principal layer is divided into many thinner ones. A similar though less regular appearance is seen in an ordinary fibrinous coagulum, but acetic acid causes the latter to swell and the stratification to disappear, which is not the case with echinococci membrane.

A sediment is commonly found at the bottom of the cyst, consisting chiefly of egg-shaped formations of carbonate of lime, as deposited in the body of the animal. They only appear in old animals and their number increases with age. These are of great value in the diagnosis, but are not absolute.

The hooklets are like young hyoid bones in shape, and are the surest indications of the entozoon when it is no longer present. One hook and a few lime formations are sufficient for a diagnosis. The cysticercus has similar but much larger hooks and likewise lime deposits.

The local treatment of cystitis in women.—Dr. Braxton Hicks communicates his views of this subject to the 'British Medical Journal,' July 11th, 1874, and after stating that he has little faith in drugs given by the mouth as a direct means of cure, proceeds to describe the local treatment. In this connection we may mention that we have recently treated three or four cases of cystitis in females after an ocular inspection of the affected parts by means of simple instruments devised for the purpose, an account of which will shortly appear in the 'British Medical Journal.' Dr. Hicks says, "the first point to make out is whether the urine be acidulated or have become alkaline . . . . . Supposing it is alkaline and ammoniacal, then proceed in this way. Take a well-oiled gum-elastic catheter open at its end, and pass it gently into the bladder. Draw off the urine, but be careful only just to enter the bladder, and when the bladder is on the point of being emptied, withdraw the catheter just without the neck. This prevents the mucous membrane from flapping down on its end. Then inject warm water slightly acidulated with either nitric, hydrochloric, or acetic acid (vinegar does very well); if nitric or hydrochloric acid be used, two drops of the strong solution to one ounce of water suffice. Directly the patient complains of a desire to micturate, let it flow out. Repeat injection till the bladder seems clear of phosphates and mucus. About half a pint of the acidulated water will generally suffice. Then inject one grain of morphia dissolved in one ounce of water, withdraw the catheter quickly, and tell the patient to retain the injection as long as possible. It is a very rare instance if this single application do not produce very marked benefit. In very acute cases the introduction of the catheter requires tenderness and care, because of the great sensitiveness of the urethra and lower bladder. By only introducing the catheter just within the bladder, much pain is saved. The exact distance can, at the first passing, be marked on the instrument. To avoid the contact of the instrument with the vesical coats, Dr. Hicks always injects without entering the bladder, and if the injections are to be withdrawn, then the instrument can be made to enter, but only so far as necessary to allow the fluid to flow out. As the urine must be drawn off at least twice daily, the treatment should be repeated if the case be severe the same number of times.

It is undesirable to pass the instrument more frequently than absolutely needful on account of the urethral irritation produced by catheterisation; but frequent micturition, itself, causes soreness or abrasion of the urethra, so that if we succeed in mitigating the frequency of urination we shall compensate for this, and this is effected by the treatment described. If the urine be not strongly alkaline a simple morphia injection may do, but by first washing out the bladder with warm water or warm solution of Pot. Permang.

three times as strong as that generally used, the cure is much expedited. After some days of this treatment try a three to four grain solution of Pot. Chloratis; use plenty, draw it off and inject the morphia, the dose of which must be regulated according to the constitutional effect produced. Dr. H. has never seen any unpleasant symptoms from two grains, but if blood appear in the urine it is a sign of some abrasion, and in such case the morphia is more likely to become absorbed. "The more we use, without affecting the system much, the better; locally subduing the nerve irritation and the tenesmus of the bladder, and the crushing of the mucous membrane which the contraction produces.

As the acute symptoms subside use more astringent washes, such as two to three grains of tannin in one ounce of warm water, or three or four drops of solution of Ferri Perchlor., using morphia immediately afterwards. If the urethra be very tender, omit all treatment for a day or two, and if, after a fortnight, the main symptoms be subdued, but the urethra very tender, apply an astringent directly to it. This may be done by introducing a bougie or catheter, covered with tannin, made very smooth, and dipped in gum-water, or a probe on which a film of Arg. Nit. has been fused. Pain, of course, succeeds, but on its subsidence patients are much relieved. This urethral treatment must be avoided in the acute forms of cystitis unless there be much irritation of the passage. Later on other remedies may be tried, such as fused anhydrous Zinci Sulph., or mopping the urethra with solution of Ferri Perchlor.

When cystitis has become chronic the injection of a solution of Arg. Nit., five grains to ten, or even fifteen to the ounce, is of much service. The latter strength caused severe pain for a short time, but much relief followed. Two grains of morphia must afterwards be left in. This treatment may be repeated a week later, and instead of the silver, Ferri Perchlor., ten minims to the ounce, may be used. In the chronic stage it is well to intermit the treatment, to see how far the local interference may be keeping up the irritation. In the acute stages the warm hip-bath and warm sponges to the genitals are not to be omitted, and general treatment must be attended to.

This method of treatment is very beneficial in cases of paralysis; and in malignant disease the simple injection of acidulated warm water gives amazing comfort, removing the phosphates and ammonia, and when to this is added the morphia a wonderful comfort is felt. So much relief is felt that the presence of a large calculus in the bladder is almost entirely unfelt if morphia be daily injected.

Irritability commonly exists for a considerable period after cystitis has lasted some time. This is much lessened by occasional morphia injections; but when it is due to the contracted state of the bladder, which cannot be quite overcome by the will, the patient should be instructed to try and hold her urine, because such exercise increases the power of the sphincter, and consequently the capacity of the bladder. In such cases we may often succeed by gently distending the bladder with warm water and repeating this treatment till the capacity and tone of the bladder and sphincter are increased.

Endometritis cured by the continuous galvanic current.—Dr. T.

Clark, of Bristol ('Brit. Med. Jour.,' Feb. 27th, 1875), relates the following case:—S. H—, æt. 39, married and had four children. After her fourth confinement she suffered severely from leucorrhœa, bearing-down pains, tenderness in left iliac region, and dysmenorrhœa. This lasted six months, when she was able to go about the house. On examination, the fundus uteri was found lying obliquely and to the left, the os uteri patulous and superficially ulcerated; and during the introduction of the fluid a porter-coloured very offensive fluid flowed from the os. Under the local use of Arg. Nit., and the internal use of Pot. Bromide, iron and sambul, with rest, she became very much better, and reported herself well. In the November following she caught a severe cold and all the symptoms returned. She was now confined to her bed and no treatment seemed of use. She was ordered away, and to continue the iron and Pot. Bromid., and Arg. nit. was applied at intervals of a week. In August of the ensuing year Dr. Clark introduced a partly insulated wire into the uterine cavity, and attached it to the negative pole; the positive was connected with a moistened sponge and applied over the pubes for 20 minutes. This was repeated three days, after which treatment the offensive discharge ceased and never returned, thus effecting that for which this method was undertaken, viz. altering the secreting property of the uterine mucous membrane. It was some months after that the hysterical condition subsided, but now, two years after, she is and has been perfectly well and is able to perform all her usual household duties.

Three cases of successful removal of fibro-cystic tumours of the uterus.—Dr. Thomas Keith, of Edinburgh, relates the history of 2 of these cases in the 'Lancet,' of May 15th of this year, and his account will deeply interest gynækological surgeons. We need not occupy space by giving the cases, as no doubt they are now familiar to all interested, but will just remention one or two points of interest which occur in the course of their narration, and conclude with Dr. Keith's remarks. Attention is drawn, in describing the first case, to a paper by Dr. Foulis in the February, 1875, number of the 'Edin. Med. Jour.,' which tends to show that secondary epithelial growths on the peritoneum may originate from escaped epithelium elements from burst cysts (simple? or originally carcinomatous?) and set up exhaustive peritonitis. The stump in this case was too large for any clamp and after the bladder was freed downwards, a steel wire was secured round the lowest part of the uterine neck, just above the vagina, by Koeberle's instrument. The healthy right ovary was not removed, but the tumour was cut away and the cavity of the uterus was opened and found to contain a small polypus. The wound was closed tightly round the neck, a strong soft needle bent in the middle securing the whole outside. The growth weighed eleven pounds.

The tumour in Case 2 was dusky brown and covered by enormous veins. It was attached to the right iliac fossa, right lumbar region, and partly to the abdominal wall. The hand could not be passed into the pelvis below the tumour. More than sixty ounces of dirty

muddish serum were removed from the large cyst without much diminishing the size. The hand was then passed behind, and with one strong sweep of the arm all adhesions were broken up, the incision enlarged, and the tumour turned out of the abdomen with the left ovary. In a few minutes the patient became faint from rapid loss of blood. The pedicle, *i.e.* the neck of uterus, was treated as in the former case. The broad ligaments were very vascular, but only the left ovary was included in the ligature. From the previous elevation of the cervix the stump was secured in the lower angle of the wound with less tension than in the previous case. Several bleeding points high up near the posterior margin of the liver, where the adhesions commenced, were secured with much difficulty. The first part of the operation occupied only a few minutes, but it was upwards of two hours ere the wound was closed. A glass drainage tube was left, passing down to the lowest point in the pelvis. The patient was pulseless when placed in bed, and the pulsation did not return in the left radial for upwards of half an hour. The canal of lymph, which after a few days forms round the drainage tube, seemed to have led to the adhesion of folds of intestine at angles, and thus became the occasion of much distress and of no small danger to the patient. As in the previous case, the slough extended beyond the wire, and a very large cavity was left on its separation, which had quite cicatrized in five weeks.

In the first of these cases there was a mistake in diagnosis, the only one in 194 operations, as hitherto Dr. Keith has had the good fortune to avoid cases of soft fibrous or fibro-cystic tumours of the uterus, but as such a mistake in diagnosis may occur to any one, Dr. K. never goes to perform ovariotomy without being prepared to remove the uterus if necessary. In this case, fortunately, the surgical treatment required was the same, as large fibro-cystic tumours of the uterus often kill as rapidly as do ovarian tumours.

These are the only cases in which Dr. Keith has interfered with uterine tumours by abdominal section, and they are enough to satisfy him that the removal of an enlarged uterus and ovaries is an operation not to be lightly undertaken. The personal attention afterwards necessary was, in each case, greater than in six average cases of ovariotomy; while in the third case there was such profuse secondary hæmorrhage seven days after the operation, that had not Dr. Keith, by mere accident, been on the spot almost immediately after the alarm was given, the patient must have died in a few minutes.

Dr. Keith deprecates the removal, by abdominal section, of uterine growths, even those that are pediculated and surgically very tempting, as our experience has not been encouraging, and also because the greater part of uterine tumours, if not malignant, give little trouble; yet there are a certain number of cases in which the patients lead useless, dependent, miserable lives, full of suffering which ends only with life, and it is for this class of case that he expresses the hope that the time is not far distant when many of these unfortunates will look to surgery for relief with as much confidence as those afflicted with ovarian disease now do.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.D., F.R.S.

1. Toxicology.

On poisoning by the smoke of tobacco.—The subjoined is the record of a case in which death was induced by the excessive smoking of tobacco. The facts came under our own observation. The patient, who was between thirty and forty years of age, was a small but well-built man, of very active habits, and reported to be exceedingly diligent and able in commercial pursuits. Previous to the occurrence about to be narrated he had been very much worried with business affairs, and had, no doubt, been trying to carry out labours which, if not beyond his own natural powers, at all events taxed them severely. He returned from business in the city of London one evening extremely fatigued, took a very slight meal, smoked several cigars and cigarettes—the precise number being unknown—and retired to bed. During the night he was heard moving about as if restless, and early the next morning he rose to work. He dictated some letters and other documents to his private secretary, and, declining all food, recommenced to smoke a cigar. He continued this process of smoking and dictating during the greater portion of the day, taking no food and, except a small quantity of brandy and water, no drink. In a period of less than twelve hours he had smoked forty cigarettes and fourteen full-sized, full-flavoured cigars. As the evening advanced he became prostrate, excited, and restless, refusing food and only sipping at drinks. Late at night he still refused to take either nourishment or medicine. He agreed, however, to give up smoking, and he went to bed and essayed for a few hours to sleep: he could not sleep, but continued excited and restless.

Early on the following morning he came for the first time under the observation of the reporter. He was at this time half dressed and out of bed. He was in a state of continuous muscular tremor, and when he moved across the room to reach his bed his legs seemed bent or bowed as if he were permanently deformed, which, however, was not the fact. He had evidently lost the full use of the muscles of the lower extremities, and he climbed into bed with considerable difficulty. When he reclined in the bed a continued movement of the muscles of the arms and legs was kept up, and the facial muscles moved occasionally in an involuntary manner. The pupils were widely dilated and the eyelids were in persistent movement, half voluntary. The tongue was dry and dark red in colour. The surface of the body was cool (96° Fahr.), clammy, and perspiring. The speech was faltering and the mind uncertain. The pulse was soft, full, and feeble. Both sounds of the heart were clear, but the action was relaxed and occasionally intermittent. The respiratory sounds, at this period, were clear, but the respiratory

movements were irregular and sighing. The breath and the cutaneous secretion were oppressively charged with the odour of tobacco.

There being no direct antidote yet discovered for the poison of tobacco, the treatment was directed to sustaining the powers of the patient and to promoting the elimination of the poison. Of course all further resort to the cigars and cigarettes was forbidden, and fluid nourishment was ordered as freely as it could be given, together with small quantities of alcohol; it was directed that the temperature of the room should be sustained at 70° Fahr., with free ventilation; and in order to sustain the process of elimination full doses of the solution of acetate of ammonia were administered every four hours. It was also carefully observed that the urinary bladder was kept free of distention from urine, a practical point of great importance in the treatment of all forms of narcotic poisoning.

The food and medicine prescribed for the patient were much objected to by him, his mind being entirely listless and every exertion seeming to be a source of annoyance. Still, he took and retained some food, and under its influence became warmer. But as the day advanced he gradually grew more and more unconscious, the muscular paralysis increased, extending from the lower limbs to the arms; every now and then there occurred a faint general convulsion, and the breathing became noisy and embarrassed. In the evening the patient was quite unconscious; his bronchial passages were filling with condensed watery fluid; he was unusually cold; there were constant jactitating movements of the muscles, and the collapse was complete. Death took place a short time after this change. A post-mortem examination could not be obtained, but the final mode of death was evidently by asphyxia from the accumulation of fluid in the pulmonary bronchial tract.

We have not unfrequently observed in confirmed smokers a series of symptoms identical with those recorded above, except in the matter of severity and in immediate result. The symptoms tally, in the main, also with those we have seen induced in the lower animals by the action of nicotine, and they were, we believe, due entirely to the poisonous influence of that alkaloid. The action was distinctive; all the muscles that are under the involuntary nervous control were paralysed; the semi-voluntary muscles were partly paralysed; and the centres governing the volitional powers were so distended that their functions were perverted. Together with these changes there was a reduction of the animal temperature owing to an interference with the chemical changes of the body, and thereupon the final catastrophe succeeded of condensation of fluids in the pulmonary organs, a catastrophe common as a direct cause of death in those who are under the effects of other narcotic poisons than nicotine, as, for instance, chloral hydrate.

The great question that remains to be solved is the treatment of cases such as the one described. In a case of poisoning by tobacco, in a boy who smoked and chewed tobacco for the first time, we found an emetic of signal service as an adjunct to treatment by the employment of external heat and the administration of a stimulant.

But in the treatment of a confirmed smoker an emetic would only depress the more, the stomach being in fact paralysed by the action of the poison. We have previously found the general plan that was adopted in the instance narrated—that, namely, of sustaining the external temperature of the body, promoting free elimination by the skin, keeping the urinary bladder properly relieved, and adminstering food with a judicious supply of alcohol—a perfectly successful method. On the present occasion all these measures proved unsuccessful, and would, we believe, be again unsuccessful in so extreme a case. It has occurred to us since that perhaps transfusion of blood might have saved the life, and in another emergency of a similar kind, we should propose to give it a trial.—*Original Report to Brit. and Foreign Med.-Chir. Rev.*, July, 1875.

Researches on strychnia.—Dr. D. Vitali indicates that the most brilliant reaction of strychnia is that which results from its treatment with oxidizing agents. A minimum quantity of the alkaloid dissolved in concentrated sulphuric acid with the addition of a small proportion of bichromate of potassa, of binoxide of lead, of peroxide of manganese, of ferro-cyanide of potassium, or of permanganate of potassa, evolves the magnificent blue-violet color, which after a time changes into a red wine tint, and then turns to a reddish yellow. Among these oxydizing substances the bichromate and permanganate of potassa distinguish themselves by the readiness with which they produce this reaction. Not long since Wenzel suggested a new method of rendering the same test still more sensitive and certain. By adopting the modification proposed by him strychnia may be discovered *in a liquid more than nine times diluted.* For this purpose he advises the use of permanganate of potassa dissolved in sulphuric acid in the proportion of one part of the permanganate in two hundred of the acid. A few drops of this liquid produce instantly, incalculable minute traces of strychnia, by the above-mentioned colorations.

As regards the readiness and rapidity of the reaction obtained by this modification no doubt can be raised. But Vitali considers it ought to be noted that its author, when announcing it, passed over one circumstance in silence which should be remembered as of the highest importance. Wenzel does not tell us whether the strychnia or the substance suspected of containing strychnia ought to be found in the solid state in order to be well assured of the result. Hence, wishing to judge from the underlined words above referred to, it would appear that it was at least indifferent. That is not the fact, since, passing lightly over that condition, the risk might be run of believing in the presence of strychnia, when in reality, in the liquid under examination, there does not exist the slightest trace. This is the reason. The solution of the permanganate in the concentrated sulphuric acid is of a green colour. But when it is diluted with water this coloration disappears to give place immediately to a vivid violet tint, which might be thought to proceed from strychnia even when that may not be the case.

Wishing to explain how it happens that the sulphuric solution of

permanganate of potassa, green in itself, becomes of a violet colour in the presence of water, Vitali says first of all that the sulphuric acid, reacting upon the permanganate, would render the permanganic acid free from it according to the equation—

$$2\left(\begin{matrix}Mn\,O_3\\K\end{matrix}\right\}O\Big)+\left.\begin{matrix}SO_2\\H_2\end{matrix}\right\}O_2=\left.\begin{matrix}SO_2\\H_2\end{matrix}\right\}O^2+2\left(\begin{matrix}Mn\,O_3\\H\end{matrix}\right\}O\Big)$$

But the permanganic acid, being unable to exist in the presence of an excess of concentrated sulphuric acid, a body greedy of water, changes itself into permanganic anhydride, which is the cause of the green colour of the liquid. In fact,

$$2\left(\begin{matrix}Mn\,O_3\\H\end{matrix}\right\}O\Big)-\left.\begin{matrix}H\\H\end{matrix}\right\}O=\left.\begin{matrix}Mn\,O_3\\Mn\,O_3\end{matrix}\right\}O$$

The permanganic anhydride, then, when water is added to the sulphuric solution, changes itself into permanganic acid, the formation of which is the cause of the change of the green tint into a violet tint.

$$\left.\begin{matrix}Mn\,O_3\\Mn\,O_3\end{matrix}\right\}O+\left.\begin{matrix}H\\H\end{matrix}\right\}O\;2\left(\begin{matrix}Mu\,O_3\\H\end{matrix}\right\}O$$

He considers the publication of the present note useful in order to prevent any sinister equivocation into which an inexperienced person might possibly be led, who, having recourse to Wenzel's method of reaction, might not have calculated on the precautions suggested in it.—*Dioscoride Vitali. Annali di chimica,* No. 3. Fascicolo di Marzo, 1875.

Oxygen as an antidote to phosphorus.—Professor Thiernesse and Dr. Casse make mention of a proposition made by Rommelaere of using the essential oil of turpentine as an antidote for phosphorus. At the meeting of the 25th of April, 1875, of the Royal Academy of Medicine, this proposition was under discussion. The experiments of Vigier and Currie, reported by Currie, should have demonstrated that it is not the purified essential oil of turpentine which exhibits an antidotal virtue against phosphorus, but really the oxygenated essential oil of turpentine, because the former will not combine with the phosphorus, whereas the latter will produce a combination which, even in not very large doses, robs the phosphorus of every poisonous effect.

It is the oxygen presented in the ozonic state by the essence of turpentine which exerts the oxydizing action upon the phosphorous poison. This is confirmed by the results obtained by Crocq upon animals poisoned by phosphorus, in which the blood manifested itself as in a pitchy state, and reacquired its normal properties under the action of oxygen or of oxygenated water.

The following results reported to the Belgian Academy of Medicine were obtained from twenty-two experiments with oxygen upon animals. They indicate that oxygen is really endowed with an anti-

dotal power, as regards phosphorus introduced into the circulation: it neutralizes the pernicious effects of the phosphorus in the blood as well as in the organs which ordinarily undergo serious mischief from the presence of the poison.

In order to obtain the desired end, it is necessary, however, to infuse the oxygen in large quantity, and shortly after the absorption of the poison, that is, before the poison has produced serious changes in the organism, for when the phosphorus has deoxydized and put to death the blood-globules the poisoned animal is fatally doomed. This is the reason, in many cases, why the poisoned subject cannot be saved even when injected with defibrinated blood charged with oxygen.

Out of twenty-two experiments nineteen gave favorable results when the poisoned animal could be injected with a sufficient quantity of oxygen. The three unsuccessful results may be explained by the circumstances, either that the poison was injected in the form of phosphorous oil in too large doses (viz. 10 grammes of oil, containing 20 centigrammes of phosphorus, into a dog weighing 10½ kilogrammes), or that the quantity of oxygen infused was too weak.

The doses of phosphorus employed were generally from 16 to 20 milligrammes, at least, for every kilogramme of the weight of the animals', and in the larger number of the cases in which the results were favorable the phosphorus had been administered under the form of a yolk-of-egg emulsion, which ought certainly to have aided in the absorption of it.

The experiments performed are all in favour of the intravenous infusion of the oxygen against poisoning by phosphorus, even when used in large doses and associated with materials which facilitate its poisonous action.

Professor Melsens, invited by Professor Thiernesse, confirms the opinion that it is not the essence of turpentine, but the oxygen it contains, which is the real agent endowed with the antidotal property against phosphorus; that oxydized essence of turpentine possesses the property of burning and oxydizing many substances, mineral as well as organic, which oxygen does not ordinarily attack; that it acts after the manner of ozone; that it transforms, even at a distance from the air, arsenic into arsenic acid and phosphorus into phosphoric acid; that it attacks divided silver, converting it into acetate of silver in the presence of acetic acid. Its powerful oxydizing properties display themselves perfectly on a sulphuric solution of indigo; it discolours this in the same way as does chlorine. Phosphorus, burned by the active oxygen of the essence, is changed into phosphoric acids, which are in fact inoffensive bodies in comparison with the corresponding quantity of poisonous phosphorus. [*Annali di Chimica*, fascicolo No. 5, Maggio, 1875. *Bulletin de l'Académie Royale de Méd. de Belgique*, t. ix, 3rd série, No. 1.]

On poisoning by aconite and chloroform.—Dr. John Ellis Blake reports a very important and rare case of poisoning by a mixture of aconite and chloroform. A young lady who was on a visit at New York

took by mistake something more than one drachm of a mixture containing equal parts of tincture of aconite-root and chloroform. Her own carelessness must be held solely to blame for mistaking the bottle containing the poison for another which held the simple medicine she intended to take; for the one containing the poison was plainly marked "aconite and chloroform," "poison," "liniment." The mistake was made about half-past eleven o'clock a.m. on February 2nd, 1875. The burning taste of the chloroform caused the error to be detected at once. The patient herself immediately took a potion of mustard and water, and then, accompanied by a friend, walked to Dr. Blake's house, a distance of one eighth of a mile. Allowing five minutes for this walk, it is assumed that the patient was before Dr. Blake in from fifteen to twenty minutes after she had swallowed the poisons.

So soon as the recital of the accident had been given, the author administered a full emetic dose (from fifteen to twenty grains) of sulphate of zinc. No vomiting followed upon this, so the stomach-pump was used, and nearly two quarts of water were passed through the stomach. The water returned smelling strongly of chloroform and coloured by the tincture of aconite. Much of the aconite was removed by the pump, but sufficient had been absorbed to produce effect. Numbness of the cheeks and tongue began to be perceived before the operation of evacuation of the stomach was finished, and this numbness rapidly extended over the shoulders and back and down the arms. Some stimulants which the patient was able to swallow were administered, but soon afterwards embarrassment of the respiration was manifested, and the patient, articulating with great difficulty the words, "All is dark now, doctor, I cannot see at all," at once became unconscious. Dr. Blake observes specially on the rapidity with which the poison, once commencing to act, overwhelmed life. Within an hour her breathing became imperceptible, she was growing cold, and she was pulseless. *No pulse whatever could be detected even in the axilla, and she remained without any trace of pulse for a period of over three hours.* The treatment now pursued was to keep the head low and to maintain artificial respiration. The artificial respiration was kept up by Faradic electricity, passed in powerful currents through the muscles of respiration. One electrode was applied on the right side of the neck, the other "near the diaphragm over the solar plexus." A deep inspiration followed, and by breaking and closing the current about eighteen or twenty times a minute the action of natural breathing could be imitated. The current used was very strong, strong enough at each application to twist the neck and body to one side, yet it utterly failed at one time, during the first hour to excite respiratory action, and Dr. Blake, who was now ably and devotedly assisted by Dr. T. Gaillard Thomas, was obliged to resort to Marshall Hall's "ready method" of artificial respiration, happily with success. By these means respiration was sustained for several hours, but there were periods when it was more difficult than at other periods to maintain it. "The lethal influence," the author remarks, "which was benumbing the nervous system,

although never seceding entirely, yet at varying intervals came rolling in upon this young life, as it were, in a great wave, and at some times it was hard to say if that life had not been utterly overwhelmed." It was observed also that in order to excite respiration the head had to be kept very low, the tendency to syncope being great, and continuing even when consciousness had returned.

About the end of the first hour the symptoms were most critical. The temperature of the body within the mouth was between 95° and 96° Fahr.; there was cyanosis of the face and the same symptom extended to the nails. At this point of the case Dr. Thomas suggested the administration of pure oxygen gas mingled with common air. A cylinder of oxygen was therefore procured and was used in the following manner. A small india-rubber tube was connected with the copper reservoir; the other end of the tube, terminated by a small nozzle, was inserted in one nostril, leaving the other nostril free to admit common air. A small jet of gas being now permitted to escape, on closing the galvanic circuit the lungs were filled with this highly oxygenated air. Keeping up artificial respiration with this additional agent, good effects were soon seen in a lessening lividity of the face and a less corpse-like look generally. The amount of oxygen used was four hundred gallons.

After a period of from three to four hours, dating from the time when the pulse ceased and when nothing but a feeble and uncertain flutter could with difficulty be heard, at times, over the region of the heart, and after artificial respiration with the oxygen had been maintained about two hours, a feeble thrill at the wrist could, to the great joy of the operators, be detected. It was not much, it was not constant, but it was sometimes there, and that was a great source of hope.

By five o'clock in the afternoon, that is to say, five hours and a half after the poison had been taken, the patient was so far restored to consciousness that she could articulate a word or two feebly. It was believed now that the battery might be stopped and the natural respiration be relied on. This, says the author, was well nigh a fatal error. Before six o'clock a crisis of unusual severity occurred, and although both the battery and the oxygen were at once put into action there was a time when it was feared they had failed. The tendency at this period was towards death by coma. The patient's condition became as follows. There was a strong tendency to sleep, which, if permitted, showed a sure and rapid tendency to pass into a death-like coma. The pulse would grow more and more feeble, and disappear altogether. To the numbness of the skin had succeeded a state of hyperæsthesia of a sensitiveness so acute that she could be roused by a current from the battery so feeble as to be almost imperceptible to others, but sinking back at once into coma on its discontinuance. When so roused she generally gave a sharp scream and uttered some expression of distress. The action of the battery was kept up constantly all night; if discontinued for more than ten minutes the pulse would flag and coma supervene.

Dr. Blake asks at this point of his narrative, "Whence this

tendency to fatal coma?" He considers that it was indirectly the effect of the aconite and chloroform, but directly resulted from the state of the kidneys from paralysis of the excretory function of those organs due to the action of the poisons. A specimen of the urine of the patient obtained at 8 p.m., showed that the kidneys had scarcely acted at all during the day, and the secretion was loaded with albumen and with fragments of casts.

At five o'clock on the following morning, the kidneys having resumed their activity, the sleep became natural, and the use of the battery was discontinued after it had been in almost constant use for sixteen hours. The specimen of urine at this time obtained was copious in amount, and was loaded as before with albumen and fragments of casts, but in time this condition passed away. Dr. Blake adds that in the course of treatment warmth was applied to the surface of the body, and that as enemata were not retained he injected hypodermically thirty minims of cognac six times in the course of the afternoon. On February 13th the patient was convalescent. She ultimately recovered.—*New York Medical Journal* for April, 1875, vol. xxi, No. 4.

[The case here recorded by Dr. Blake is, indeed, full of interest, as illustrating once again what can be effected by continuous artificial respiration. In the patient whose history has been given, the action of the heart was reduced to that lower tension of action which we have described in the Croonian lecture "On Muscular Irritability after Systemic Death," as the tension of syncope. The heart-stroke in such cases is sufficient to keep up a feeble pulmonic circuit and a sufficient systemic circuit to prevent pectous changes; but the nervous centres are not effectively supplied, and the respiration therefore fails to be excited. In such examples, a little assistance to the respiration is a means of maintaining the circulation of low tension for a very prolonged period, and when the depression of the heart is from an agent that is susceptible of elimination from the body under the continuance of vital motion, the artificial respiration becomes a means of restoring life that is even startling in the grandeur of its results.

In recording Dr. Blake's report, with sincere admiration of his and his colleagues' courage, endurance, and skill, we would venture to make an observation or two derived from experimental and clinical experience bearing upon the details of the treatment. We think that in another similar case, after the stomach has been emptied, it would be advisable not to administer alcohol. Alcohol diluted assists to distribute the alkaloidal poisons the more actively through the tissues, and to hasten the symptoms of poisoning in all their intensity. Then in respect to artificial respiration, we would submit that in the cases under consideration, it is better to maintain it by the double-acting bellows, that is to say, by force supplied by another person than by the force of the patient excited into motion by the electric stimulus. We have tested the two methods by careful experiment on the deeply narcotized animal, and, by means of the metronome regulation of the Faradic current, have

made the respiration excited by the current as regular as the ordinary natural respiration of the animal. By this means, it is true that the artificial respiration can be long sustained, but it is at the expense of the force of the animal, and is, therefore, exhausting to an extreme degree. When the artificial respiration is sustained by the operator, it is equally effective as an artificial aid, and it relieves the patient of all exhaustion. Between the two processes, there is, in short, just the difference that occurs between stimulating a worn-out animal to walk to a destination, and carrying it to the same destination. Thus, in our experiments, we found we could even destroy muscular action by the current through the exhaustion it produced, and when we succeeded in restoring by it it was always with the result of an after-debility and drowsiness which is unknown when the simple hand method is employed. Once in Dr. Blake's case, at the period when he and his learned colleague had to resort to Marshall Hall's plan, this exhaustion of muscle under the galvanic stimulus did occur, and the rest from that stimulus with mechanical maintenance of respiration was the saving modification of treatment.

The employment of oxygen as an adjunct to artificial respiration, on which Dr. Blake naturally dwells with so much emphasis, introduces to our notice a practical matter which we have tried many times to study by experiment. We have no doubt that the use of the oxygen in the diluted form, as suggested by Dr. Thomas, was of service. At the same time, if oxygen be not at hand, no operator need be under any anxiety from its absence. We have found that to raise the temperature of the common air supplied by artificial respiration to 75° Fahr. is as effective, in quickening the oxidation of the blood, as is increase of oxygen. Indeed, on the whole, the warm air method is safest, because an excess of oxygen, the precise degree of which excess is not yet defined, is actually injurious.

We incline altogether to the view expressed by Dr. Blake that the secondary symptoms of collapse and coma were due to a uræmic condition. For this very reason we would urge the more strongly the importance of simple mechanical artificial respiration by the double-acting bellows—through which no waste of tissue or formation of secondary product of tissue is involved—to the muscular respiration excited by galvanic stimulus.

We fail to see the value of the hypodermic injection of the cognac, but we do not, in the absence of inquiry into it, by direct appeal to experiment, dispute its value. On the whole we have rarely met, in twenty years of reporting on toxicological subjects, with a record more valuable to medical science than this which Dr. Blake has supplied. As a result it is one of the practical fruits of experimental research which every true investigator must recognise, especially at this moment, with extremest satisfaction.]

On the presence of alkaloids in decomposing organic substances and in the bodies of the dead. — We have noticed in a previous report the fact that some Continental authors have been testing the tissues of the dead for evidences of alkaloidal bodies which have

formed spontaneously in the processes of decomposition. The inquiry has been further pursued by M. Schwanert, who has obtained from decomposed human livers, spleens, and intestines, a non-crystalline alkaloid, which cannot be obtained from fresh corpses. This substance was obtained in quantity sufficient for some little examination from the organs of a corpse that had remained sixteen days at about 30°, and was quite decomposed. The viscera, either alone or mixed with zinc chloride, were repeatedly treated with alcoholic tartaric acid, and the extract thus obtained purified by the Stas-Otto process, and then shaken with ether; excess of alkali being then added the new base was extracted from the liquor by ether. By evaporation, at as low a temperature as possible, the substance is left as a yellowish oil which does not solidify, and possesses a peculiar odour recalling that of propylamine, but has no bitter taste; it volatilises completely on long standing or when heated, and changes red litmus to a deep blue; it forms a crystalline deliquescent hydrochloride, readily volatile, readily soluble in water, less so in alcohol, and developing white vapours of a peculiar unpleasant odour on addition of soda-ley.

Sulphuric acid dissolves the hydrochloride, forming a colourless solution, gradually becoming a dirty brown colour after standing, and greyish-brown on warming: the colourless solution yields, on warming with sulphuric acid solution of sodium molybdate a beautiful blue shade, which gradually becomes green; with potassium dichromate it becomes first red brown and afterwards grass-green. Nitric acid dissolves the compound, producing a yellow colour.

The platinum salt is a dirty-yellow precipitate consisting of microscopic six-sided stars, and contains 31·35 per cent. platinum. Gold chloride gives with an alcoholic solution of the hydrochloride an amorphous yellow precipitate; mercury chloride a white crystalline precipitate; potassio-mercuric iodide a dirty white, and iodised potassium iodide a clear brown precipitate. Tannic acid gradually renders the liquid turbid; sodium phospho-molybdate gives a yellow precipitate, clotting together and becoming blue with ammonia.—*Deut. Chem. Ges. Bet.*, vii, 1332, and *Journal of the Chemical Society*, March, 1875.

[The subject here broached is not new, as some of the recent experimenters seem to suppose, but is one of those experimental inquiries which observers in England have for many years past had under investigation. So far back as 1862 Dr. Letheby pointed out that organic decomposing bodies yield, not only ammonia, but also other volatile nitrogenous substances which are peculiar to organic decomposition. Dr. Stenhouse, as Letheby has shown, described in 1849, in speaking of the products of decomposition of nitrogenous organic matters, that whenever ammonia is generated in large quantities from complex substances, either animal or vegetable, it is always accompanied by the formation of a larger or smaller amount of volatile organic bases.

Dr. Grace Calvert discovered, in the volatile alkalies of some putrefying flesh, complex chemical bodies yielding carbon, hydrogen, nitrogen, phosphorus, and sulphur. Dr. Odling detected in similar

substances complex volatile alkalies containing carbon, and we ourselves in 1865 isolated from the serum derived from the peritoneal cavity of the human subject an alkaloidal body which possessed poisonous qualities found to be communicable and to which we gave the name of *septine.* It is but just to claim for the English school the priority in these advances of science before they are retranslated here from foreign sources and reproduced, as they assuredly will be, in a manner intended to convey the idea of an origin anywhere rather than in the English mind.]

II.—Forensic Medicine.

Survivorship after injuries to the head.—Some of the most curious cases on record, in a forensic point of view, are those which relate to survivorship after injuries to the head. Dr. Day, of Stafford, has enriched medical literature with a most interesting record of a case in which a man travelled on foot a long journey (from Stafford to London) with a portion of a gun-lock lodged in his cranium. Some further valuable illustrations bearing on the same class of injuries are recorded by Mr. Robert Harvey in a report of the Madras legal returns received from civil surgeons of the Bengal Presidency during the years 1870-73.

The question, says Mr. Harvey, as to how long a patient could have lived after a fatal injury to the head is frequently asked in courts of justice, and is of practical importance. The point is mentioned in too few cases to allow of any fixed rules being laid down, but a sufficient number is given to show that one opinion, which is not unfrequent, viz., that a certain injury must have caused instantaneous death, can very seldom be justified, numerous cases of life being prolonged for many days after very severe injuries are recorded. A few have been incidentally noticed already in connection with inflammation within the skull. Mr. C. L. Fox, civil medical officer of Muzuffargarh, gives the case of a Mussulmani, aged 25, in his return for August, 1872. She had been struck three blows on the head with a plough-handle as thick as a man's wrist. All the bones of the face, except the lower jaw, were fractured; the lower part of the parietal bone, both orbits, and the lesser wings of the sphenoid were comminuted, many fragments being driven into the brain, which also protruded from the external wounds; she died on the fifth day. A man at Delhi lived seven days after receiving blows from a blunt weapon, which produced a compound fracture of the lower jaw and extensive fracture of the base. Another at Kohat died of cerebritis ten days after a fracture through the left orbit into the base. Two men at Sitapur lived, one 16, the other 17, days after extensive fractures extending into the nose. In a case at Jhilum a man hit on the head on the 2nd of April, 1870 sustained a fracture of the left parietal and frontal bones, involving the orbit. Head symptoms came on the 4th, and he was trephined, dying next day. Surgeon J. P. Wright gives a good case in the Nursingpur return for December, 1870:

Subject, a Brahmin, aged 60; admitted on the 18th November

with ten contused wounds on the head, apparently caused by a blunt cutting weapon. Fracture of outer table of skull to right of forehead. Partially unconscious, but could be roused with difficulty. Began to improve on 22nd, and went on well till 1st December, when "his appetite fell off and he began to lapse into insensibility, not complete, but of evil omen." Several pieces of bone were removed, but he continued to sink, and died on the 7th of December. Post-mortem examination revealed "extensive fracture of the frontal bone with an abscess formed in the brain and considerable effusion of black blood on the surface. The separated portions, four in number, had been removed during life, and the brain protruded through the vacant space. The injuries are quite sufficient to cause death, and it is only a matter of surprise that he lived as long as 19 days."

In the Mirat return for November, 1870, is the case of a man who died of compression of the brain six days after an injury which caused extensive fracture of the vertex with effusion of blood on and beneath the membranes. He had left hemiplegia, and was insensible throughout. A man at Maimansing lived three days after a starred depressed fracture at the fronto-parietal point, with blood on the brain. He also was comatose, as in all probability most of them were.

In other cases the patient lived a few hours only. A man at Gujranwala lived 16 hours after a blow from a stick, which separated the coronal suture, and another at the same station 16 hours with depressed fracture of the vertex, and separation of the sagittal and right lambdoidal sutures. Another at Pind, Dadan Khan survived 26 hours with a depressed fracture of the skull and laceration of the brain; and a man at Muzaffargarh ten hours after a similar injury. The most remarkable case is one by Mr. E. P. Roberts, of Raniganj. A Hindu, aged 35, was hit on the head by a fragment of a burst boiler. "The right side of the skull had been driven in, and a large portion of the brain forced out," yet he lived for an hour and a half. Similar instances might be multiplied, but those given are sufficient to show that patients may survive for long periods after head injuries, and do in fact seldom die instantaneously. In some of the cases where death was reported as immediate, a fact is no doubt recorded, but in many it is an opinion merely, and the following shows how easily mistakes may be made. A civil surgeon in the north-west provinces, finding a transverse fracture extending from the vertex down to and right across the base in a man who had been felled by a *lathi* blow from behind, reported that "death must have ensued instantaneously." It was proved on the trial, however, that in spite of the very serious injury he had received, he had lived about an hour after being carried home. No harm was done in this case, and death, though not instantaneous, was very rapid; but had the man lived for some days, as it is obvious from some of the cases above quoted he might have done, serious discredit might have attached to the medical officer.—*Indian Medical Gazette,* April 1, 1875.

III.—Hygiene.

On the origin of cerebro-spinal meningitis.—Dr. Henry B. Baker, of Lancing, Michigan, reports to the State Board of Health upon an epidemic in Monroe and Lenawee counties, in Michigan, and adds to his report a study of some other facts relative to the cause of this disease. The author has collected evidence concerning eighty-eight cases. An abstract of this report has already been published, and was the subject of comment in this country as well as in America last year. We shall, therefore, now only notice certain of the new facts that are embodied in this elaborate report. (1.) Dr. Baker disputes the propriety and correctness of the name given to this epidemic disease. The lesions of the cerebrum and of the spinal cord which are usually found may, he thinks, be secondary only. He inclines, on the whole to accept the name suggested by Dr. Rodenstein, and to call the malady "*tetanoid fever.*" (2.) The definition of the disease by its symptoms he supplies from Dr. Clymer:—"It is an acute specific disorder, commonly happening as as an epidemic, general or limited, and rarely sporadically, caused by some unknown external influence; of sudden onset, rapid course, and very fatal. Its chief symptoms, referable to the cerebro-spinal axis, are—great prostration of the vital powers, severe pain in the head and along the spinal column, delirium, tetanic and occasionally clonic spasms, and cutaneous hyperæsthesia, with, in some cases, stupor, coma, and motor paralysis, attended frequently with cutaneous hæmic spots; its morbid anatomical characters being congestion and inflammation of the membranes of the spinal cord, particularly the pia mater, although there is reason to believe that the evidence of these changes may be wanting even in cases of long duration." (3.) On the subject of the communicability of the disease, Dr. Baker states that the majority of observers have come to the conclusion that the disease is incommunicable from the sick to the healthy. But a minority hold a different opinion, and he cites certain details which, as far as they go, corroborate this view. His own observation is that contagiousness of a like character to that of smallpox, scarlet fever, or typhus, is not possessed by the malady under consideration. The question is—Does epidemic meningitis, like typhoid fever or cholera, possess a peculiar contagiousness of its own, a property of communicability peculiar to itself? This question still remains to be solved. (4.) The symptoms which characterise the cerebro-spinal meningitis, or, properly speaking, tetanoid fever, are more correctly and fully described in this essay than in any we have previously seen. The records are from the pens of six physicians, namely, Drs. Paquette, S. L. Jones, G. W. Jackson, M. Wilcox, C. T. Southworth, and H. C. Wyman, all evidently most careful and thoughtful observers, and all residents and attendants of the sick in the affected districts. From these records it is clear that the epidemic showed differing shades of character and intensity in the area of its temporary locality. The symptoms varied from those of an ague to those of an extreme tetanus. From these learned observers we find

that in the remarkable epidemic they witnessed the leading symptoms were as follow :—The symptoms varied somewhat in different cases according to the period of time in which they were in progress, but they represent a more perfect whole of the phenomena of the malady than has yet been recorded. At first there was fever, which often ran high, but was not always persistent; in some examples, indeed, the fever was so intermittent that the earlier attacks resembled ague. Upon the fever there followed delirium, which was sometimes associated with deafness. The eyes were commonly affected with squint, which was generally convergent, but now and then divergent. The eyes were glassy, with a peculiar stare; the lids did not freely raise or lower, they did not wink with natural frequency: The pupils were generally dilated, but they contracted under the influence of opium. There was always pain in the head, headache usually most severe in the back of the head and extending down the back. The head was often drawn back, and in a few instances there was lock-jaw. The mouths were dry. These were the leading phenomena affecting the head, face, and mouth.

Most of the patients suffered from vomiting, and this symptom, which came on early, was often very troublesome. The vomited matter looked like chopped grass in water. The bowels, as a rule, were constipated. The surface of the body was tender to the touch, the tenderness being most marked over and along the spine. The skin was generally spotted, the eruption consisting of bluish evanescent spots which disappeared on pressure. The temperature of the body was irregular, it was sometimes raised at first, and then would become lower than natural. Dr. Jackson notes that the highest temperature he observed was 105° Fahr. The pulse was frequently almost normal, sometimes slower than usual, but accelerated after a few days. It was very often intermittent. The respiration was irregular. The most marked symptoms of all were shown in the muscular and nervous systems. Dr. Paquette uses on this point a very quaint expression, which, we agree with Dr. Baker, embodies an important fact. He (Paquette) says, the nerves were "taut," by which he means that there was general muscular tonicity which was increased by any irritation of the skin, because of its increased sensitiveness. With this tonicity of the muscular system there occurred tetanic convulsion, sometimes locked jaw, and delirium. Dr. Southworth also names, as a characteristic symptom, a falling-in of the abdominal walls, the abdomen appearing as though it had been dug out or emptied of its viscera. The urine was not passed freely, and was of high colour, resembling coffee grounds in some cases. The duration of the disease was extremely varied. Some cases terminated fatally in so short a time as three hours. Others extended over several days.

A very careful inquiry into the cause or causes of tetanoid fever is finally recorded by Dr. Baker. He shows that atmospheric cold and the reaction which arises from it is a powerful disposing agent to the malady. He shows again that the mental condition known as fear is another equally important disposing influence. But as

regards the actual cause of the malady his argument in the main supports our original theory that the disease is a form of ergotism, that the active agent is a fungus or smut occurring in the grain, and that the bread food from such grain, eaten by the sufferers, is the source of the poison.—*Second Annual Report of the State Board of Health of Michigan*, 1875.

IV.—Summary.

On the Effects of Strychnia. By P. A. Falk, M.D. Révue Thérapeutique Médico-Chirurgicale, 15 *Juillet*, 1874.—The physiological action of strychnia, its place as a poison, and the tests by which it is recognisable, are noticed by the author, who, if he adds little that is original, writes with sound judgment and knowledge.

Diseases of the Mystics, Louise Lateau. By M. Warlomont. La Revue Scientifique, April 10, 1875.—This is a report made tohe Academy of Medicine of Belgium on the whole case of the stigmatic bleeding and fasting girl of the village of Bois d'Haine in Hainaut, Louise Lateau. It is a splendid answer to the work of M. Lefebvre on the same subject. Warlomont throwing aside all the superstition, exaggeration, and we may say imposture, which surrounds this case, faces the facts on physiological and pathological grounds, and shows the pure physical nature of the phenomena.

Ragjpootana Dispensary, Vaccination, Jail and Sanitary Report for 1873. *Calcutta. By W. J. Moore, Surgeon-major. Published by authority.*—The report is short, but is an admirable condensation of a series of observations which, considering the area of the districts the author had to study and the mass of facts with which he had to deal, are truly laborious. We observe two striking notes in respect to the origin of cholera: (*a*) that human intercourse alone did not explain the irregular extension of the disease in Rajpootana in 1869, and (*b*) that the malady was extended by human intercourse after it had once originated.

The Science of Disinfection.. By John Dougall, M.D. Separate Treatise. Glasgow, 1875.—This paper was read before the Health Department of the Social Science Congress of 1874. It defines very clearly the physical difference between the processes of fermentation and putrefaction. It shows also that many so-called antiseptics have for their specific properties the power of preserving organic poisonous substances, and that to quench decomposition of organic bodies is a very different thing from the art of killing infection. Every practitioner will read this short and able paper with profit.

The Longevity of Brainworkers. By George M. Beard, A.M., M.D. Separate pamphlet. New York, 1875.—Dr. Beard assigns as the causes of the exceptional longevity of great brainworkers—1. That great men usually come from healthy, long-lived ancestors. 2. That a good constitution usually accompanies a good brain. 3. That great men who are permanently successful have corresponding greater will than common men, and force of will is a potent element in determining longevity. The one requisite of great success is "*grit.*" 4.

Great men work more easily than ordinary men. 5. Great brain-workers have not all been rich nor all been poor; the majority have been most of the time surrounded with at least moderate comforts.

The Diagnosis of Blood-stains. By Joseph G. Richardson, M.D. American Journal of the Medical Sciences, April, 1875.—Dr. Joseph Richardson in this short paper defends his previous position from an attack made upon it by Dr. J. J. Woodward in the previous issue of the above-named journal. Dr. Woodward maintains that we can never affirm truthfully on the strength of microscopical investigation that a given stain is positively composed of human blood. Dr. Joseph Richardson agrees with this as being literally true, but not the whole truth, because it often happens in practice that evidence other than microscopical narrows down the conditions of a case to the question: Is this stain human blood or that of an ox, pig, or sheep? The question thus narrowed can be answered by the microscopist according to our author, and we entirely agree with him in his opinion. We further agree with him in the caution he displayed in his former paper in not suggesting to the criminal population what bloods of inferior animals are difficult of differentiation from human blood.

River Pollution, with a special reference to the Pure Water Supply of Towns. By Jabez Hogg. Reprint from the Journal of the Society of Arts, May 12*th*, 1875.—Mr. Hogg, in his interesting paper, argues that the use of the large supplies of water stored up in the deepest recesses of the earth will alone remove the evils of water supply under which we labour. Such water stores are absolutely pure and brilliant. They are entirely free from all suspended matters, from decaying organic substances, and fæcal refuse. Neither spores, seeds, fungi, ova, nor the larvæ of animalcules can possibly penetrate to the deep recesses of the earth. Deep well water contains probably the inorganic products of the thunderstorm of a century or more ago—a little lime dissolved out of the chalk as it passes through the rock, a little chloride of sodium, washed out of the sea ages gone by and deposited in the soil, but it contains no ammonia in organic combination, and no albuminoid ammonia. It contains what we prefer, fixed air, which, while it imparts a briskness to it, renders it in no way unsuited for domestic and dietetic uses.

BOOKS, PAMPHLETS, &c., RECEIVED FOR REVIEW.

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Clinical Lectures on Diseases peculiar to Women. By Lombe Atthill, M.D. Third Edition. Dublin, Fannin & Co. 1875. pp. 294.

St. George's Hospital Reports. Edited by J. W. Ogle, M.D., and Timothy Holmes, F.R.C.S. Vol. VII, 1872-4. London, Churchills, 1875. pp. 396.

Consumption in Australia. By C. E. Reeves, M.D. Melbourne, J. Brooks. 1874. pp. 154.

On Leprosy and Elephantiasis. With plates. By H. Vandyke Carter, M.D. Lond., H. M. Indian Service. Published under the sanction of the Secretary of State for India. London. 1874. Large 4to., pp. 248.

On Life, and on Vital Action in Health and Disease; being the Lumleian Lectures. By L. S. Beale, M.B., F.R.S. London, Churchills, 1875. pp. 110.

Manual of Instructions for the Guidance of Army Surgeons in testing the range and quality of vision, and in distinguishing the cause of defective vision in soldiers. By Surgeon-General T. Longmore, C.B. Second Edition. London. 1875. pp. 134.

On Paralysis from Brain Disease in its Common Forms. By H. Charlton Bastian, M.D., F.R.S. With illustrations. London, Macmillan & Co. 1875. pp. 340.

Anatomy, Descriptive and Surgical. By Henry Gray, F.R.S. Seventh Edition. With an Introduction on General Anatomy and Development. By T. Holmes, M.A. London, Longmans. 1875. pp. 788.

On Diet and Regimen in Sickness and Health; and on the Interdependence and Prevention of Diseases, and the Diminution of their Fatality. By Horace Dobell, M.D. Sixth Edition, revised and enlarged. London, Lewis. 1875. pp. 248.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1875.

Analytical and Critical Reviews.

I.—The Present Position of Antiseptic Surgery from a Practical Point of View.

What is "antiseptic surgery" in the sense in which Lister and those who have endeavoured to understand and follow out his process and method understand the term? This is by no means a very easy question to answer.

In the first place it is not a wild, energetic, and indiscriminate use of carbolic acid as a lotion, an unguent, a paste, or a bandage. No amount of carbolic acid, unless used under special precautions and with special aims, can of itself make a dressing antiseptic in Lister's sense of the word. The keynote of his method is struck when the surgeon says I am to aim at the

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[1] 1. *Cases illustrative of the Antiseptic Use of Carbolic Acid.* By Joseph Bell, F.R.C.S. Edinburgh, 1869.

2. *Address in Surgery at Annual Meeting of British Medical Association held at Plymouth, August,* 1871. By Joseph Lister, F.R.S.

3. *Lister's Lectures in 'British Medical Journal,' January 14th,* 1871.

4. *An Inquiry into the Theory and Practice of Antiseptic Surgery.* By James Cumming, M.D., M.R.C.P.E., L.R.C.S.E. Edinburgh, 1872.

5. *Langstaff on Hospital Hygiene,* 1872.

6. *Du Traitement des Plaies en General et en Particulier d'un Mode Nouveau de Pansement Antiseptique.* Par le Docteur Louis Beau de Toulon, 1873.

7. *Sulla Medicatura Antiseptica alla Lister in Chirurgia.* Pel Dr. Gesualdo Clementi. Catania, 1874.

8. *Volkman on Subcutaneous Osteotomy.* 'Edin. Med. Journal,' 1875.

9. *Papers on Antiseptic Surgery.* By Joseph Lister, in 'Lancet,' *passim,* 1875, and in 'Edin. Medical Journal,' Sept. 1875.

10. *Annandale's New Operation for Knock-knee.* 'Edin. Med. Journal,' July, 1875, p. 19.

11. *Klinische Ergebnisse der Lister'schen Wundbehandlung und über den Ersatz der Carbolsäure durch Salicylsäure.* Von C. Thiersch. Volkmann, 'Sammlung klinischer Vorträge,' Nr. 84 and 85.

On the Clinical Results obtained by Lister's Treatment of Wounds, and on the Substitution of Carbolic Acid by Salicylic Acid. By Professor C. Thiersch, of Leipzig. Volkmann's 'Collection of Clinical Lectures,' Nos. 84 and 85.

prevention of putrefaction in a given sore, abscess, amputation wound, or compound fracture. I am not to be satisfied by deodorizing it or concealing it when it has once begun. Carbolic acid may be, and doubtless is, often with advantage used as a deodorizer, in the same way that permanganate of potash in solution is most valuable in a ward or applied to a sore fetid with the gases of putrescence. It may make an excellent stimulant and caustic to a sloughing chancre, and act in its own way, as nitric acid or bromine do, with the best effects; but in none of these ways does it act as an antiseptic dressing in the sense in which Lister uses the word.

Nor is it necessary for an antiseptic dressing that carbolic acid be used at all. There are other agents which have been used, and others may be discovered, far better for the purpose. Carbolic acid has many inconveniences and even disadvantages. We believe that many of the misconceptions regarding the mode of treatment, and much of the want of confidence felt in its results, have arisen from the confusion in the minds of surgeons between carbolic acid as a general panacea for surgical ills, and the special use which Lister has made of it as a convenient vehicle in his antiseptic dressing.

Let us inquire, then, first, What is the principle on which true *antiseptic surgery* is based? 2nd, What are the methods by which he endeavours to carry out and apply this principle? and this second head will demand a little historical retrospect into the past as well as a description of the present; and, 3rd, What are the results obtained by Lister and others? There will also open up to our mind a fourth most difficult question to settle—we fear almost impossible with our present knowledge, and yet one for which the profession must ask for an answer: *i. e.* Is this great theoretical discovery to become a practically useful one; is mankind to be the better for it; can it be used in private as well as in an hospital; is the ordinary practitioner, in his country rounds, bound to make himself the slave of a most elaborate and expensive and time-consuming mode of dressing his patients? If he does not do so is he diminishing their chances of recovery? If he does use it is he alleviating their sufferings, shortening their sickness, and hastening their cure?

These are questions to the conscientious and painstaking surgeon of almost infinite importance; the responsibility is great in trying to answer them; and alas! as yet the data are few. While theoretical castles in the air raise their heads into an ether in which all germs have been either killed or rendered harmless, the steady foundation of well-recorded cases, with accurate observations of pulse and temperature, are few and far between. Isolated cases, wonderful in their loneliness, general

conclusions, dealing in the vaguest manner with hospital hygiene, tell us too much or not enough. "Pyæmia and hospital gangrene have been banished from the hospital since the antiseptic methods have been used;" indeed, very gratifying, but not so remarkably confirmatory of the value of antiseptics, if they have been banished equally from the wings of the hospital in which antiseptic surgery has been used and those in which it has not.

In this short notice we can do little more than group together, in as short and compact a way as possible, a few of the scattered materials we have on which to form an estimate of the method, value, and results of the surgical dressing known as Lister's antiseptic dressing.

The principle on which the antiseptic method of treatment is based may appear to be comparatively unimportant; it might be supposed to be sufficient to carry out the details aright. Not so, however, is it in the estimation of Professor Lister. While we do not find him saying that the method must stand or fall into Pasteur's theory, yet, in his opinion, it is on this theory that the antiseptic system of treatment is based. In July, 1868, in an address delivered before the Medico-Chirurgical Society of Glasgow, in directing the attention of the society to an experiment founded on one of Pasteur's, but with slight modification, he says—

"On this theory the antiseptic system of treatment is based, and I venture to say, that without a belief in the truth of that theory no man can be thoroughly successful in the treatment. If any one believes that putrefaction through atmospheric influence is due to the operation of the atmospheric gases alone upon the putrescible materials, he will be perpetually meeting with the most perplexing anomalies, and will be liable to commit the most serious practical blunders; the truth being that on the one hand the most complete exclusion of the gases of the air affords no security against the occurrence of putrefaction, and that on the other hand the freest admixture of air into the putrescible contents of a wound or abscess will fail to induce putrefactive changes, if the germs if that air have been removed by filtration or deprived of vitality by a germ poison."[1]

Again, in 1871, Mr. Lister gives still stronger expression to the same opinion, and, to avoid repetition, and, at the same time, to state fairly his own views, we must allow him to speak for himself as to the importance of the principle:—

"Among the causes which have hitherto interfered with the general acceptance of this mode of treatment, by far the most prejudicial is the doubt of its fundamental principle instilled by various

[1] 'British Medical Journal' for July 18th, 1868, p. 54.

authors who have opposed the germ-theory of putrefaction, and who, supposing themselves to be advocating the cause of truth, have not only, as it appears to me, espoused the side of error, but have unconsciously inflicted an amount of material evil upon their fellow-creatures such as mere speculative opinion is seldom able to produce. For few medical men in active practice have the leisure to sift and weigh the facts and arguments of such a discussion; yet, if they lose firm faith in the guiding principle of the treatment, the attainment of a full measure of success becomes with them a matter of impossibility. 'Felix qui potuit rerum cognoscere causas' was never more applicable than here."

And next as to the facts on which the principle is founded:

"Those of which I have first to speak have reference to the well-known experiment of Pasteur of boiling a putrescible liquid in a flask with an attenuated and contorted neck. It is now nearly four years since I introduced portions of the same specimen of urine into four glass flasks, so as to make each about one third full, and, after washing their necks, drew them out with a spirit-lamp into tubes less than a line in diameter, and then bent three of them at various acute angles, while the fourth was left short and vertical, though equally narrow. Each flask was then boiled for five minutes, the steam issuing freely from the orifice; after which they were left with the ends of the necks still open, so that air might pass in and out freely in obedience to the condensation and expansion caused by the diurnal changes of temperature. The boiling, I need hardly say, was for the purpose of killing any organisms contained in the liquid or adhering to the side of the glass: the bending of the necks in three of the flasks was with the view of intercepting particles of dust, which, according to the germ-theory, are the cause of putrefaction, as distinguished from the atmospheric gases; while the fourth neck was left short and vertical for the sake of contrast, to afford opportunity for dust to fall into the liquid, where such portions of it as had the nature of living organisms might propagate and induce in the fluid any changes of which they were capable. The result was, that in the vessel with short and upright neck two different kinds of fungi, visible to the naked eye, soon made their appearance, and these grew steadily till they had attained large dimensions, the liquid meanwhile gradually changing from its pale straw colour to a deep amber tint, implying alteration in its chemical constitution. But in the flasks with bent necks the fluid remains to this day entirely unaltered.[1] I regret that the distance from Edinburgh to Plymouth is too great to permit me to bring these objects before you. One perilous journey they have already had, when I took them from Glasgow to Edinburgh nearly two years ago, nursing them carefully during the railway journey, to the amusement of my fellow-travellers; and in the drive from the station

[1] Some minute shining crystals have of late been deposited on the bottom of the flasks, probably from condensation through the very slow evaporation constantly going on.

to my house the violent rocking of the vehicle churned up their contents till the upper part of the body of each flask was full of a frothy mixture of the putrescible liquid with the atmospheric gases; yet no harm resulted, and the fluid in the bent flasks still retains its original pellucid clearness and pale hue. Bringing these in imagination before you, as represented in this diagram, consider what these facts imply. Let us not push them one tittle beyond their inevitable interpretation. The drops of moisture deposited in the bent tubes from condensation of the steam when the lamp was removed dried up in a few days, so that the necks have been for nearly four years open and dry from end to end. Comparing the capacity of the part of the body of the flask containing air with that of the narrow neck, it is manifest that a considerable portion of fresh air has passed into the flask every night, in consequence of the fall of the temperature, a corresponding portion passing out again by day, though not the same which entered; for the diffusion of gases would ensure its mixing freely with that previously present. Hence, during nearly four years this putrescible liquid, this boiled urine, has been freely exposed to the influence of the atmospheric gases, yet it has not putrefied. About half a year after the commencement of the experiment I decanted a little of the liquid from one of the bent flasks into a wineglass, and found it sweet in odour and faintly acid to test-paper, while an honest search with a powerful glass failed to detect even the minutest organism. Covering the glass to prevent evaporation, I found it in two days stinking, while under the microscope it already teemed with various organisms, and a few days later it showed fungi to the naked eye. Thus the fluid was demonstrated to be still putrescible and a favorable nidus for organic development; yet both these changes have been prevented for nearly four years by the circumstance that the air, in gaining access to it, had to pass through a narrow bent tube of clean dry glass. Now such a tube could not by possibility arrest any atmospheric gas. It cannot possibly have stopped anything but the atmospheric dust. It follows, therefore, not as a matter of theory, but as an inevitable inference from fact, or, in other words, as a truth, that, so far as this particular instance of a putrescible liquid is concerned, both the development of such organisms as the microscope enables us to detect, and the concomitant putrefactive changes, are occasioned by particles of dust suspended in the atmosphere, but not by the atmospheric gases. I confess, Mr. President, I am ready to blush for the character of our profession for scientific accuracy when I see the loose comments sometimes made upon this experiment; and I am tempted to doubt whether some of the commentators can have enjoyed the advantages of sufficient education either in chemical physics or in logic. The simplicity and perfect conclusiveness of the experiment constitute its great charm, and render it, as it appears to me, deserving of your careful consideration. Yet, having before published an account of it, although nearly two years have since elapsed, so as to add considerably to its weight, I do not know that I should have felt justified in bringing it forward on the present

occasion, if I have not an additional fact to communicate respecting it besides the results of further lapse of time. We have seen that we have been forced to the conclusion that, though the gases of the air certainly pass into the body of the flask and out again every twenty-four hours, its dust, even though of extreme minuteness, must be arrested by the contorted tube. Now, inevitable as this inference is, it will be satisfactory to have it converted into the position of an observed fact. This Professor Tyndall's simple but beautiful mode of investigation with a condensed beam of light has lately enabled me to do. Having prepared two dry glass flasks, one of them having the neck drawn out and contorted, I arranged them, through the kind assistance of my colleague, Professor Tait, so that the body of each was pierced by a beam of highly condensed sunlight in an otherwise dark apartment. The beam, scattered by the floating particles of dust, showed white in the surrounding darkness, within the flasks as well as without, proving that the air within the flasks was dusty like that outside. I now closed with sealing-wax the orifice of the unbent flask, and, leaving the other open, allowed both to remain undisturbed in the laboratory. A fortnight later I again submitted them to the solar beam, condensed as before, and now found that in both flasks alike the visible part of the beam terminated abruptly at the glass on each side, showing that in both the air was, as Tyndall expresses it, "optically empty," or, in other words, that it was destitute of even such minute particles of floating matter as could produce the faintest nebulosity. During the time between the two observations, the force of gravity had led to the subsidence of even the minutest floating particles; and, though the changing temperature of the laboratory had of necessity induced the daily entrance of air into the open flask, the bent form and fine calibre of the tube by which it was admitted had effectually filtered it of suspended material, though in a very dusty apartment.

"The other class of facts in this division of the subject to which I am anxious to direct your special attention was also suggested by one of Tyndall's experiments with the condensed luminous beam—that, namely, in which he proved the perfect manner in which cotton-wool filters the air of its suspended particles, by blowing against the beam with a pair of bellows having a mass of the cotton tied over the nozzle; the result being that the beam, elsewhere white from illuminated dust, became perfectly black at the part on which the current was directed through the cotton-filter: hence the idea naturally suggested itself that cotton-wool might be used with advantage as an antiseptic dressing. Of course it would be useless to apply ordinary cotton without special precautions, for, according to the germ-theory, putrefactive particles must exist among the fibres and lie scattered over the wool. But if the cotton were impregnated with some volatile material capable of destroying the vitality of the septic organisms, and then placed upon the wound after washing it with a lotion containing the same substance in solution, the result ought to be, supposing the theory true, that, after the volatile antiseptic had become dissipated by diffusion from the dressing and from

the wound, the cotton-wool, though destitute of any chemically antiseptic properties, should effectually prevent, by its filtering property, the access of any putrefactive agents, and keep the wound sweet, while in itself a perfectly bland and unstimulating application. Accordingly I prepared four samples of cotton-wool by diffusing through each one of the following substances—chlorine gas, sulphurous acid gas, carbolic acid vapour, and the vapour of benzine—four materials very dissimilar in chemical properties, but having a common hostility to low forms of life. Chlorine, sulphurous acid, and carbolic acid, are well known to have such a property; and, knowing that benzine is used by the entomologist for killing insects, and having ascertained by experiment the potency of its vapour for the destruction of pediculi, I thought it probable that it would also answer our purpose. I then dressed with these four kinds of prepared cotton-wool various suppurating sores, excoriations, and contused wounds, after washing the surface with the corresponding lotion, or, in the case of benzine, with the undiluted material. The results in every instance corresponded exactly with theory. After about twenty-four hours' exposure at the temperature of the body, the cotton-wool was found to have lost the odour of the antiseptic, yet the blood, serum, or pus, as the case might be, remained perfectly sweet for an indefinite period, while healing advanced in the satisfactory manner that might be anticipated from the absence of all irritating quality in the dressings. There was, however, one circumstance, highly instructive in itself, which interfered sadly with the utility of this application; namely, that, if the discharge happened to be sufficiently copious to soak through the cotton-wool and appear at its external surface, putrefaction occurred throughout the entire mass of the moistened part down to the wound, even within the first twenty-four hours after the dressing, if the fluid were sufficiently copious to penetrate within that period. It is only when dry that cotton-wool can arrest the progress of microscopic organisms, which have ample room to develope among its meshes when filled with a putrescible liquid.

"And now, gentlemen, allow me, at the risk of seeming tedious, to endeavour to bring home to you a little more closely the inference that is to be drawn from these facts. But, first, let me describe in detail the manner in which the dressing with carbolated cotton-wool was practised. The cotton-wool having been impregnated with about a two hundredth part of its weight of the acid in the form of vapour, the surface of a granulating sore or abrasion was washed, together with a portion of the surrounding skin, with a solution of the acid in about forty parts of water. A piece of oiled silk of the size of the sore was then applied, to prevent the dressings from sticking through dryness. Over this was placed a piece of folded linen rag, rather larger than the oiled silk, and impregnated with the carbolic acid vapour in the same manner as the cotton-wool; the object of the rag being to absorb the discharge and prevent it from trickling down, as it was otherwise apt to do, below the slightly absorbent cotton, involving its early appearance at the surface and

consequent spread of putrefaction to the wound. Lastly, a well overlapping mass of the carbolised cotton-wool was securely fixed by a bandage. The result, as before stated, was that, though all chemical antiseptic virtue left the dressing within a day or two, putrefaction was excluded by the cotton-wool for any length of time, provided the discharge did not penetrate to the exterior of the mass. Consider, now, the circumstances of the serum or pus that oozed from beneath the edges of the oiled silk into the folded rag; let us suppose a week after the application of the dressing, when all traces of the volatile antiseptic had certainly disappeared. Here was a highly putrescible liquid, not subjected to boiling, as in the flask experiment, or acted on by any chemical agent whatever, yet remaining free from putrefaction in a rag moistened with it at the temperature of the human body, simply because it was covered over with pure dry cotton-wool. How, then, did this cotton-wool exclude the causes of putrefaction in the atmosphere? It certainly did not keep out any of the atmospheric gases. The same cause that led to the escape of the volatile antiseptic necessarily occasioned a perpetual intermingling between the external air and that between the meshes of the fabric, as any one acquainted with Graham's beautiful researches into the laws of gaseous diffusion must at once admit. The only constituent of the atmosphere which the cotton-wool would possibly exclude is its dust; and this we know, from Tyndall's experiment, it did exclude. Here, then, we have another inevitable inference from fact, another truth, and that in itself all-sufficient, with reference to the antiseptic system of treatment; the truth, namely, that pus, blood, and the dead tissues in contused wounds do not putrefy through the influence of the atmospheric gases, but through the operation of particles of dust, which may be permanently deprived of septic energy by the vapour of an agent like carbolic acid. I do not ask you to believe that the septic particles are organisms. That they are self-propagating, like living beings, and that their energy is extinguished by precisely the same agencies as extinguish vitality, such as heat and the various chemical substances to which I have referred, is certain, and is of the utmost practical importance. But if any one, in spite of these facts, and in spite of the strong analogy of the yeast-plant, and the various kinds of fungi which we term mould, prefer to believe that the septic particles are not alive, and to regard the vibrios invariably present in putrefying pus or sloughs as mere accidental concomitants of putrefaction, or the results, not the causes, of the change, with such an one I, as a practical surgeon, do not wish to quarrel. Nor do I enter upon the question whether spontaneous generation can take place at the present day upon the surface of our globe. To do this would be to engage in doubtful disputations which I promised to avoid.

"But I do venture earnestly to beg of all of you who are engaged in surgical practice, that you will give these simple facts your careful consideration; and if you think the interpretation I have given a sound one, do not let any statements, whether in books or in journals, shake your belief in the truth that putrefaction, under

atmospheric influence, as it occurs in surgical practice, is due to particles of dust ever present in the atmosphere that surrounds our patients, and endowed with wonderful chemical energy and power of self-propagation, yet happily readily deprived of energy by various agents which may be employed for the purpose without inflicting serious injury upon the human tissues. With this as your guiding principle, you will find yourselves successful with the antiseptic system of treatment; but without it, whatever theory you adopt, you will ever be walking in the dark, and therefore ever liable to stumble."

Having thus briefly described the principle on which antiseptic treatment is founded as far as possible in the inventor's own words, we will now give a brief historical *résumé* of the stages through which the use of carbolic acid as an antiseptic agent has passed in the hands of its inventor and his disciples, describing at some length and in full detail the *present* mode, and then, before passing on to discuss results and objections, give a brief notice of one or two other antiseptic agents recently introduced, either as handmaids to or supplanters of carbolic acid.

As an illustration of the gradual manner in which the antiseptic *method* has been evolved, and the numerous changes through which it has passed and may still have to pass, though the *principle* always remains the same, we may give a brief abstract from Dr. Cumming's excellent thesis, of the various ways in which abscess has been opened and dressed antiseptically between 1866 and 1871. These, again, may be contrasted with the present method, described afterwards.

In 1866 a piece of rag, soaked in an oily solution of carbolic acid, one part of acid to four of oil, is laid over part to be opened. The upper edge of this being kept in position by an assistant, the lower edge is raised by the surgeon, and his knife, previously dipped in the same oily solution, is plunged into the abscess, under the curtain of the rag, which is at once dropped over the aperture as the knife is withdrawn. The pus is then squeezed out from under the rag, a piece of lint soaked in the same oily solution put in the wound to keep it open, and the wound is then covered by a piece of block tin, on the under surface of which a layer of carbolic paste or putty, a quarter of an inch in thickness, is spread. (This was the oil mixed with prepared chalk, and seems to have been both a dirty and troublesome application.) The tin was laid on with adhesive plaster, and a folded towel fixed over all so as to absorb discharge, and secured by a bandage.

At every subsequent dressing new pieces of rag, block tin, putty, &c., were to be used.

(Both faith and patience must have been needed, and must have been often severely tried, to tempt a surgeon in this nineteenth century to work away with such troublesome and filthy applications.)

1868.—A double layer of plaster (lined with gutta percha and brushed over with bisulphide of carbon) was substituted for the paste dressing.

(This is all Dr. Cumming tells us of the plaster, so we do not know its constituent parts.)

1869.—Additional cautions are needed. Anoint the neighbouring parts well with oily solution and remove any hairs. The piece of lint as drain is to be soaked in a weaker solution, 1—10. A new preparation, consisting of shellac 1—3 (?), is applied as a dressing, overlapping the wound by several inches, fixed by pieces of adhesive plaster two and a half inches in breadth (applied equally on the plaster and the skin). A syringe charged with carbolic lotion is used in changing the dressings, the nozzle to be inserted beneath the edge of the plaster to refresh the wound. Sinuses to be injected with a lotion of 1 part of carbolic acid to 5 parts of methylated spirit. When the discharge has diminished a weaker shellac plaster may be used, 1—6.

May, 1871.—Antiseptic gauze introduced as now in use, instead of the lac dressing, changed under the protection of dropping from a syringe containing a 1—40 solution of carbolic acid in water.

Thus there has been a double line of improvement steadily going on in (1) means used for purifying air and preventing entrance of germs *during* an operation, and (2) in the means of dealing with the inevitable soaking of discharges, blood, serum, or pus, *after* the operation. For the first, the lint or rag soaked in carbolized oil of 1866 has improved into the syringing of 1869-71, the hand spray-producer of 1871-75, up to the steam spray-producer of 1875; while the dressings have altered from oiled rag, putty, or paste on block tin, shellac plaster, up to antiseptic gauze. So far all the alterations seem to have been improvements, though still to many surgeons the complexity of the dressings and the constant supervision and care required will, we fear, act as stumblingblocks to their acceptance of the method.

Lister's Practice in 1875.

To do justice to his method it is still necessary to give details, for success depends on the closest attention to every step in the process. Let us take the mode of dealing with an abscess, a compound fracture, and an amputation respectively.

1. *An abscess.*—Let us suppose a psoas or lumbar one which

must be opened. The skin in the neighbourhood, after the most careful washing, is still further to be purified and deprived of any septic organisms by a thorough sponging with a saturated watery solution of carbolic acid (1—20). The dressing is to be prepared, drainage tubes of various sizes at hand, a piece of oil-silk protective, and some pieces of carbolic gauze, in a basin with 1—40 solution, with which also a syringe is ready loaded. The knife to be used and any other instrument required are all to be previously soaked in the 1—20 solution.

The dressing is composed of eight, twelve, sixteen, or even thirty-two layers of the carbolic gauze.[1] This is intended to absorb, and at the same time to render harmless, all the discharges which are to come from the abscess between the first and second dressings; but lest these discharges should soak through all the layers, and thus reach the external air, and thence absorb septic organisms, there is put between the outer layer of the dressing and the one next it a layer of some impervious substance (macintosh cloth). The dressing should be large enough to overlap the wound to be made, for many inches in every direction, and its size should to a certain extent be proportioned to the amount of discharge expected and its nature.

All being ready, and the patient, if need be, thoroughly under chloroform, the part, hands of operator and assistants being again soaked in the solution, the antiseptic spray is set to work over the part, either by hand-spray producers, or, what is far better, by the steam-engine lately introduced for the purpose. This spray should reach the part in a state of fine division, and not weaker than 1—40. The knife, previously dipped, should then be used to open the abscess freely, so that all its contents, not merely pus, but fragments of connective-tissue, masses of coagulated albumen, &c., be freely evacuated without pressing or undue squeezing. The incision should be large enough to avoid all risk of retention of subsequent discharge and consequent inflammatory tension. A drainage tube of sufficient calibre is then to be inserted into the abscess, long enough to reach to the deeper parts of it, yet not so long as to curl up or exert any pressure. Its orifice should be flush with the surface, and it is to be prevented from falling into the abscess and being lost, by pieces of knotted carbolized threads, which are to be left outside.

A good illustration of the method, defects, and dangers of any mistake in the use of drainage tubes is found in the following account of the progress of a case in which Mr. Lister recently

[1] For a full description of the appearance and preparation of this material, see the 'Lancet' for March 13th, 1875.

opened into an inflamed knee-joint with antiseptic precautions and inserted a drainage tube.[1]

"The subsequent progress of this case has illustrated well the remarks made at the demonstration, with regard to the effects of a free opening, or the contrary, under antiseptic management. When I saw the patient on the following day, I learned that he suffered unusual pain in the afternoon after the operation, which became very severe during the night, and though somewhat less in degree at the time of my visit, was still very considerable. The temperature had risen on the previous evening to 102·4° F., and was now 101·8°. Such a state of things would at one time have alarmed me, and would have made me fear that putrefaction had occurred. This, however, I felt confident could not have been the case, and another probable explanation suggested itself. The peculiar bulging above alluded to, situated over one of the pouches of the synovial capsule beside the ligamentum patellæ, had tempted me to make the opening in that situation; but the bulging part collapsing on escape of the fluid, the only way in which I could ensure complete introduction of the drainage-tubes into the joint was by passing their ends under the ligamentum patellæ; and I thought it not unlikely that they might have been compressed, and their function so interfered with. Accordingly, on changing the dressing, I found that the gauze presented a bloody stain, which appeared sufficiently accounted for by oozing from the surface of the wound while the joint was fully distended. And it appeared that the disturbance to which the articulation had been subjected had led to unusually rapid effusion from the synovial surface, and this being unable to escape, had produced great tension, attended with pain and fever. I at once placed him under chloroform, and made a fresh incision at the outer side of the limb into the pouch above the patella, and introduced a drainage-tube larger in diameter than the little finger, after pressing out the clear serous and fibrinous contents of the capsule. This was of course done with antiseptic precautions, and a dressing like that employed the day before was applied. The result was that almost immediately after awaking from the chloroform sleep, he felt himself entirely relieved of his pain; and not only has that which was induced by the first operation left him, but he has entirely lost that which had annoyed him for so long a period previously. The temperature in the evening was found to have fallen to 99° F., and has since remained normal, and the discharge, which has continued to be merely serous, has so diminished in quantity, that when I last saw him (15th August) I substituted a drainage-tube of medium size for the large one, and was able to direct that an interval of three days should be allowed to pass before the next dressing. I must add, that he has tested the limb, contrary to orders, by getting out of bed and resting his weight upon it, but without any of the pain which he had formerly experienced on so doing. In all other respects he is in perfect health."

If a drainage tube is not at hand, a piece of lint soaked in oil

[1] 'Edin. Medical Journal,' Sept. 1875, pp. 198, 199.

in 1—10 carbolic acid will make an excellent substitute ; part of it should be inserted between the lips of the wound. The oil-silk protective strip is then put next the wound, a piece of dipped gauze over it, and the large dressing over all. When next day the surgeon comes to dress the wound, there will be very little to be squeezed out of it, perhaps only a little transparent serum. The drainage tube may be withdrawn, cleansed in carbolic acid, and reintroduced with as little disturbance as possible, and new dressing applied in same manner as before, still under the protection of the spray. Day after day, soon at intervals of three or four days, this may be repeated ; the abscess cavity rapidly contracts so that the drainage tube must be shortened at every dressing and in time the abscess will heal. But till the last trace of discharge of serum is gone no relaxation in the vigilance of the surgeon can be permitted and fullest antiseptic precautions must be used, or all may go wrong. This is exceedingly likely to take place from weariness on the part of patient or surgeon in cases of psoas or lumbar abscess connected with diseased vertebræ, in which the duration of the treatment must be measured by years rather than months or weeks.

2. A compound fracture. The wound being thoroughly washed externally and if necessary the hair shaved cleanly off all the vicinity of the injured parts, the bone is replaced and adjusted, all completely loose fragments being removed and those still adherent to periosteum replaced in position. If this cannot be done without undue tension a portion of the protruded end of bone may often be removed with advantage. If recently inflicted it is then sufficient to wash out the wound carefully by a syringe loaded with 1—40 carbolic lotion. If of long standing a stronger lotion may be needed.[1]

3. An amputation. If sufficient pains be taken, an antiseptic management is both possible and may be very advantageous. Let us suppose a railway smash involving an amputation of the thigh in a muscular man, one of the most fatal of all surgical injuries. Such a case will exhibit the difficulties and disadvantages of the procedure as well as its possible gains.

Collapsed and cold as the patient may be, the first essential is

[1] For cases of compound fracture seen for the first time several hours after the accident, Mr. Lister has of late used a still stronger antiseptic in the form of one part of carbolic acid dissolved in five parts of spirit of wine, introduced into the recesses of the wound by means of a gum-elastic catheter connected with a syringe by a piece of caoutchouc tube. In this way the antiseptic is made to penetrate the coagula in the various parts of the wound more effectually than it could be by forcing it in through the external orifice, while at the same time we avoid the needless disturbance which this procedure may entail in consequence of the irritating liquid being driven for a greater or less distance through the cellular interstices of uninjured parts.—‘Lancet,’ March 13th, 1875.

the removal of his wet dirty clothing and the most careful washing of the limb above the seat of injury with carbolic lotion; the hair is to be shaved from the limb. The shattered remains of the leg are to be enveloped in a towel wrung out of carbolic acid, and confided to a dresser. The tourniquet, being put on high up, the limb and operator are to be sprayed upon in the freest manner all through the operation. The vessels, even the femoral, are to be tied with carbolized catgut; every bleeding point is to be secured, and the surface of the wound thoroughly washed with 1—40 lotion. The edges are then to be brought together with carbolized silk sutures and the whole wound must be thoroughly drained, to prevent retention of discharges, either by two, three, or more short tubes stuck in at intervals, or, in some situations more conveniently, by one long drainage tube pierced with numerous and large side-apertures drawn through the whole breadth of the flaps near its base. Just before the dressings are applied a final washing of the stump may be given by syringing through the drainage tubes with 1—40 lotion. A large dressing is then ready which (after the exact edge of the wound has been first covered by a strap of oilsilk protective and a deep dressing of dipped gauze[1]) is to be rolled round the stump, overlapping it by some inches below, and reaching up at least as far as the perinæum. It is made to embrace the limb pretty tightly and then the free end of the tube is folded so as thoroughly to close it in, and the whole affair secured in position by a large bandage made of the same antiseptic gauze as the dressing, which should be carried up as a spica bandage to take at least two or three turns round the pelvis.

We have now briefly discussed the principles and the method in which carbolic acid is used. A word may be due here to other antiseptic agents which may possibly take the place of or supplement carbolic acid; these are chloride of zinc, boric acid, salicylic acid.

Chloride of Zinc.—Chloride of zinc, a powerful escharotic, irritant, astringent, deodorizer or disinfectant, according to the strength and method in which it is used, is a useful agent in antiseptic surgery. Well known to surgeons since Mr. Campbell de Morgan's interesting paper on the use of chloride of zinc in surgical operations and injuries, it is used not only in the hope of destroying germs of cancer possibly latent in the vicinity

[1] The use of these may be best explained in a note. The oilsilk protective is put along the edge of the wound to prevent the irritating effect of the carbolic gauze on the cut surface itself. The deep dressing of dipped gauze is merely a strip of gauze previously soaked in carbolic lotion, so as at once to destroy and neutralize any germs which may have rested in the protective or dressing. That such precautions are necessary illustrates both the complexity and difficulty of the whole question.

of an operation undertaken for the removal of the original growth, but also because it has an excellent effect in preventing, or perhaps rather postponing, putrefactive changes in granulating or recent wounds.

In superficial cancerous sores it is an excellent escharotic and at the same time so far an antiseptic, that beneath its scab formed over the sore, healing may take place absolutely free from odour or contraction. Mr. Lister has pointed out that its use after Mr. de Morgan's method after excisions of maxillary bones or other operations about the face greatly diminishes or prevents the subsequent fœtor which is so often communicated to the breath and which doubtless has in it an element of danger by possible blood-poisoning. Used after excision of mamma, we have sometimes seen it followed by rapid healing, if free exit be given by drainage tube or otherwise to the large amount of salmon-coloured discharge which follows its use ; in other cases, again, differing in no very marked way, union has been delayed and attended by excessive suppuration.

Lister, at one time, seems to have used chloride of zinc in solution (40 grains to an ounce of water) as an application to putrid sinuses (by injection), but finding that if used too strongly and becoming diffused into the cellular tissue it caused sloughing he seems now to have given up its use.[1]

Though from its want of volatility chloride of zinc will not do as a permanent antiseptic dressing, still its power of preventing suppuration or putrefaction for some days after a single application to a cut surface renders it of great value as an antiseptic in many operations, as it gives time for granulations to form.

Boracic or boric acid is another antiseptic recently introduced into surgical practice in this country by Professor Lister. A full account of his obtaining the knowledge of its virtues from Dr. Stang, of Sorweg, in Norway, and of the experiments he made with it will be found in his lecture on improvements in the details of antiseptic surgery in the 'Lancet' for May 1, 1875.

He had been suffering from an onychia of the little finger, and found, as patients not unfrequently do, that a weak watery solution of carbolic acid caused almost intolerable pain, and yet did not destroy the pungent ammoniacal odour. The Swedish *amykos*, the active principle of which is boric acid, was absolutely painless and yet perfectly removed the odour. This was the starting-point of a series of experiments with it which have proved not only in Mr. Lister's hands, but in those of many

[1] 'Lancet,' March 20th, 1875, p. 401.

other surgeons, that boric acid has certain valuable qualities as a dressing.

In prurigo ani and eczema of the legs it is sometimes of great value. Stored up in lint in very considerable quantity it makes an excellent mild, comparatively unirritating antiseptic application for fetid ulcers. Such storing up is easily managed, for the acid is dissolved very freely in boiling water and very sparingly in cold; so that lint dipped in a saturated solution at or near boiling point and allowed to dry will be found to have absorbed a very large amount of the acid, and as the crystals are soft and unctuous, such lint makes an agreeable dressing as well as one which will act as an antiseptic for a considerable time even in cases where discharge is profuse.

In the 'Edinburgh Medical Journal' for Sept., 1874, a very full account is found of the method of using the acid in granulating wounds by Professor Lister, and in the discussion which followed Mr. Lister's statement Dr. Chiene stated that he had used boracic acid in ointment and powder in eczematous ulcers so long ago as 1872.

Salicylic Acid.—Professor Thiersch, who has for some time been using Lister's mode of dressing wounds antiseptically, has, within the last year, been using salicylic acid, which in March, 1874, was put at his disposal by Professor Kolbe.

This substance has been for some time known to chemists as a constituent of the oil of winter-green (*Gaultheria procumbens*), and of the essential oil of Monotropa Hypopithys, &c.

It may be made from dry powder of sodium-phenol, by conducting into it dry carbonic anhydride at 170° C. Salicylate of soda is formed, and this is decomposed by hydrochloric acid into sodium chloride and salicylic acid, which is precipitated. A good account of it is given by Dr. Squibb, in a paper read by him before the New York Academy of Medicine, and subsequently published in pamphlet form. The acid, bleached or unbleached, occurs in minute broken acicular crystals, giving it the appearance of a granular powder, which is odourless and nearly tasteless, but leaves a sweetish astringent after-taste. It is very slightly soluble in cold water, but very soluble in hot. The presence of neutral salts, as of sodium phosphate, has the power of increasing its solubility in water, three parts of the latter salt causing the solution of one part of the acid in fifty parts of water. It is much more soluble in alcohol and ether than in water. It melts at 125° C., and sublimes at 200° C.

Dr. R. Godeffroy has an interesting paper on the subject of this acid, in the *Zeitschrift des allgemeinen österreichischen Apotheker-Vereines*, April, 1875. He states that salicylic acid

may be obtained in various ways besides by the method which we have detailed above. It may be prepared by the oxidation of salicin, a glucoside contained in the barks of some species of salix. It may also be got by the oxidation of saligenin, by heating oil of gaultheria with potassium-hydrate, and by decomposing potassium-salicylate by hydrochloric acid. Salicylic acid may be considered to be an oxidation product of benzoic acid, since it contains one atom more of oxygen, thus: $C_7H_6O_2$ = benzoic acid, and $C_7H_6O_3$ = salicylic acid. It differs from its isomers, oxybenzoic acid and paraoxybenzoic acid, by giving a beautiful violet colour with solution of ferric chloride.—'Edin. Med. Journ.,' July, 1875.

The preparations used by Professor Thiersch are:

1. Salicylic water, a solution containing one part of the acid in 300 of water; or about as much as distilled water at the ordinary temperature of a hospital ward is able to take up.

2. Salicylic cotton of two degrees of saturation, 3 per cent. and 10 per cent.

3. Salicylic jute, admirably soft, a powerful absorbent and a cheap and satisfactory disinfectant.

The manner in which these are prepared is thus described in the 'London Medical Record' for June 2, 1875.

For 3 per cent. salicylic cotton, 750 grammes are dissolved in 750 grammes of spirit of specific gravity 0·830, the solution is then diluted with 150 litres of water at a temperature from 156° to 176° Fah., and the mixture is used to saturate 25 kilogrammes of cotton-wadding freed from fatty matter; different proportions of same ingredients are used for the 10 per cent. saturation. The saturation of the cotton is best done in a large shallow tub, and it is best to use only a small quantity of cotton wool at a time to allow of an equal distribution of the acid. After saturation of the cotton in successive layers, the salicylic acid crystallizes in the progress of cooling, and it is then carefully dried.

Jute dressing is prepared by 3 or 4 per cent. saturation with the addition of 20 per cent. of glycerine to prevent the acid being given off too freely in the form of dust. The jute dressing is most suitable for suppurating wounds in consequence of its being able readily to absorb fluids, in which process the cotton is comparatively deficient.

Salicylic acid has some advantages over carbolic acid if it can be found to be equally efficacious as an antiseptic. It is much less irritating; it is inodorous, and it is non-volatile, so that it can be trusted for a longer time.

As it is less irritating, it in some measure may be expected to involve less complicated dressings and manœuvres. So that

in its use, in many cases, the oilsilk protective may be dispensed with, and perhaps even the macintosh which Lister puts between his outer layer and the one next it. On the other hand, salicylic acid provokes coughing and sneezing, both as a dry dressing and when used as spray.

One advantage of immense importance it possesses over carbolic acid,—that it seems to be much less poisonous. The frequency of fatal poisoning by misadventure by carbolic acid has, of late years, been most striking and alarming.

As is the case in carbolic acid irrigation, salicylic acid will after some time be found in the urine; it indicates its presence by an olive-green colouring.

As to results Professor Thiersch prefers it to carbolic acid, and in his hand it has entirely superseded the use of the latter. The statistics he gives, as quoted in 'Medical Record' for May 26, 1875, are by no means striking or encouraging.

Of twenty-two cases reported four were amputations of the thigh with fatal results; and in two cases of amputation of the forearm, three of amputation of the leg, four of the hand, and six of the artery, the course was not remarkable—three successful amputations of the thigh and leg occurred in children. Dr. Thiersch has arrived at the conclusion (we fear, perhaps, on rather insufficient data), from a consideration of the unsuccessful cases, that, although salicylic acid may not prevent all decomposition, and although offensively smelling products of decomposition may be found, it prevents that form of decomposition which leads to pyæmia.

Lister, while he does not believe that salicylic acid will take the place of carbolic, finds salicylic acid useful in preventing decomposition in cases where the bandage has to be left on a considerable time, say a week.[1]

Risks and Objections.

We can take these in no regular order, but just as we find them cropping up as we watch the treatment or in the writings of unbelievers and critics.

Risks.—Every remedy which has potency for good has also when misapplied or abused an equally power to hurt. Carbolic acid is no exception to this rule. One of the saddest results of the great frequency of its use, as an antiseptic, as a lotion, or a deodorizer, is the alarming frequency of cases of poisoning by accidental swallowing of this too potent external remedy.

Even in the surgeon's hands, however, sad results have fol-

[1] 'Edin. Med. Journ.' for July, 1875, p. 95.

lowed its rash and indiscriminate application. On its first introduction a zealous house-surgeon after injecting a compound fracture with the strongest obtainable solutions of the glacial acid kept pledgets of lint soaked in the same solution applied to the limb, causing a most distinct and rapid caustic action which necessitated amputation. Too energetic injections of cellular tissue with a strong syringe loaded with a strong solution have produced similar unfortunate results.

Apart from these results, which may be put down to *abuse* of the remedy, in certain constitutions and in certain states of system, very marked effects followed the *use* of the drug even in careful hands. For example, in many patients, the opening of an abscess with the antiseptic precautions described above, which includes the washing out of the abscess cavity with one or more syringefuls of a 1—40 solution, is followed within a few hours by the passing of urine which immediately on exposure to air assumes the characteristic smoky tint seen after the internal use of carbolic acid.

In one case under the writer's observation this persisted for weeks, even when the strength of the antiseptic injection was much reduced, and the urine at times contained broken-down blood-discs indicating considerable renal congestion.

Still more dangerous and more marked are the distinct symptoms of general carbolic-acid poisoning of the system which have been observed to occur after the use of antiseptic dressings for extensive abrasions or destructions of the skin, as after a severe burn. Valuable as the gains in the subsequent treatment are of having a large burn rendered antiseptic we believe that carbolic acid is not an agent which can be invariably or indeed frequently used for this purpose with safety. For such cases, sulphurous, boracic, or perhaps salicylic acid will be found safer means of obtaining the desired results.

While thus acting the unpleasant rôle of *advocatus diaboli,* and stating some of the risks attending the use of this valuable drug, we must not forget a not unfrequent troublesome though not dangerous symptom, which we have frequently observed in cases (for example) of excisions of the mamma under antiseptic precautions. The irritation of the gauze sets up a form of cutaneous erythema, eczema, or even impetigo of a most troublesome kind, which goes on increasing so long as the dressing is kept on and does not disappear even for many days after it is removed.

It cannot be predicted or prophesied in any given case whether this will be the result or not; it occurs in otherwise healthy people and does not imply any previous tendency to skin erup-

tions, nor is it more frequently observed in scrofulous or syphilitic cases than in unimpeachable constitutions.

On Granulating Wounds.—On a granulating surface the action of a carbolic-acid dressing is rarely beneficial. If it is in immediate contact with the granulations, it almost invariably acts as an irritant, delaying cicatrization, by causing the formation of a peculiar yellowish layer apparently of coagulated serum and pus resembling a diphtheritic pellicle. This remains for days unchanged, and the wound beneath it, though painless and inodorous, makes no progress whatever. In fact, once a wound or ulcer has assumed a granulating surface, and deep parts are consolidated, the period at which carbolic-acid dressings are useful seems for that wound to be past, and boracic or salicylic acid may be used if antiseptics are to be continued at all, or very frequently water dressing may be substituted with advantage.

N.B.—This applies only to superficial wounds,as after excision of tumours with loss of substance, ulcers, &c., not to cases in which deep abscesses have been opened with antiseptic precautions. In these cases at the risk of irritating granulations, full antiseptic precautions must be maintained with the utmost care and accuracy till the wound is absolutely healed from the bottom and not a drop of discharge is either retained or escapes. This is specially the case when the abscess has depended on, or is connected with, the presence of dead or diseased bone, as in psoas, lumbar, or joint abscesses.

While for exposed granulations carbolic acid even with all possible precautions of protective oilsilk, &c., does not seem useful, it is far otherwise when the surgeon has to deal with a blood-clot effused between the lips of an incision as in an amputation, or in removal of a superficial tumour. If the antiseptic method be successfully carried out, the remarkable sight is witnessed of the clot *not only not suppurating,* but of its actually becoming organized, and being utilised to fill up a gap, or supply deficiency of material, under the protection of the antiseptic dressing. Those who have used it often are familiar now with the changes on the blood-clot, its partial contraction, its vascularisation in the first six or eight days, and its gradual approach in appearance and usefulness to the character of normal cutis.

A very interesting case of the using of a blood-clot with the intention from the first of filling a gap and preventing contraction we quote from a recent number of the 'Edinburgh Medical Journal:'

"*Mr. Chiene* showed a patient to the Medico-Chirurgical Society of Edinburgh, suffering from a simple malady, but whose case illustrated

the organization of a blood-clot in an open cavity. He believed that this was the first systematic attempt to fill a cavity with a blood-clot. The patient suffered from a horn on the heel, which he had removed along with a triangular piece of skin, each side of the triangle being $1\frac{1}{4}$ inches long. He had then cleared out all the tissues down to the os calcis, thus making a cavity half an inch deep. He next applied protective and a gauze dressing before removing the tourniquet, in the hope that blood would ooze from the sides and fill the cavity with a blood-clot. This operation was performed on the 18th of April. Six days after, the cavity was filled with a dark jelly-like substance. On the twelfth day this clot was of the same colour, but firmer in consistence. On the sixteenth day it bled when scratched. On the twenty-sixth it was evidently organized and red in colour. Epidermis began to cover it on the thirtieth day; and on the thirty-fourth it was completely covered and entirely healed, without any contraction. This organized blood-clot was therefore half an inch deep, and like an equilateral triangle, each side being an inch and a quarter in length. It was still a point of clinical interest whether the contraction would ultimately take place. On this point he would satisfy the Society by showing the patient in July. He only wished further to state, that no retentive apparatus had been employed, as he had no proof when he operated that there would be contraction."

Another from Mr. Lister's own practice: [1]

"The next case is one of ununited fracture in the lower part of the femur of a year's standing, in a man 36 years of age. Twelve days ago, I cut down on the outer side of the limb, a very long incision being required. Finding the fragments overlapping about an inch, I removed portions with the gouge and hammer from the posterior surface of the upper fragment and the opposing part on the anterior surface of the lower one, so as to leave two fresh osseous surfaces in apposition. Without antiseptic treatment, this would have been a very dangerous operation. The risk of pyæmia would have been so great that, in common with most surgeons, I should have regarded such interference as unjustifiable; but I think we may venture to say that, with antiseptic treatment in its present form, all such risk may be certainly avoided. It is now twelve days since the operation. For the first few days blood and serum were effused very copiously, and we had an arrangement by means of which a large mass of gauze could be applied in considerable extent under the limb. But the time has come when it might be put up in a more permanent form. This plaster-of-Paris arrangement was applied yesterday, while the limb was kept well extended by the pulleys, the patient being under chloroform. I have here a limited space for the dressing, and therefore use a correspondingly thick mass of gauze. This you will find often a matter of importance, as in operating for strangulated hernia, where you have not much space between the wound and sources of putrefaction in the perineum. And so in the

[1] 'Edin. Medical Journal,' Sept. 1875, p. 203

present case, the window left in the plaster-of-Paris is occupied by a very substantial mass of gauze. The discharge of the last twenty-four hours has caused, you see, merely a small brownish stain upon the gauze, the result of a slight amount of serum, tinged with the colouring matter of the blood. The ends of the wound were stitched up for about three inches at each side; those parts united by first intention, and are completely healed. The central part of the wound was left open for the orifices of three large drainage-tubes. And herein we see the persistent blood-clot. Two days ago, I took out for the first time the drainage-tubes, and they were, just as in the case you last saw, lying in tubular moulds in the coagulum. One of them was permanently removed; the other two were re-introduced after being considerably shortened by cutting portions off from the deeper ends. In taking out drainage-tubes you must be particularly careful to have the spray properly directed. For as the drainage-tube comes out, air must enter to take its place, and this air will be septic or not as the spray is not or is over the wound. Here we see the orifices of the two drainage-tubes, one of which may probably now be dispensed with altogether. As I remove them, you observe the tubular beds in which they lay. And here, as in the last case, we have as yet no suppuration whatever from the open wound."

Effect on Hospital Mortality and on the Health of Wards.

Here we approach the very key of the position, and as yet, we regret to say, that really valuable comparative statistics are few and far between. We have abundance of general impressions—vague splendid generalities. Some few continental authorities will be quoted immediately.

What we want are series of statistics or observations of two kinds. (1.) A list of operations, amputations, herniotomies, ovariotomies, &c., sufficiently large to be compared on the one hand with similar sets of cases in other places, or on the other with sets of cases under similar conditions except *no* antiseptic precautions. Materials for such a comparison surely cannot be wanting, and we would like to see an equal number of Lister's cases compared with Mr. Callender's magnificent results in St. Bartholomew's with his special mode of dressing stumps, or with the very carefully prepared tables of splendid successes in amputation by Professor Spence in neighbouring wards of the same hospital in which Mr. Lister works—these last obtained without any antiseptic precautions. (2.) Much would be learned from a series of careful observations of pulse and temperature of cases treated antiseptically. So far as we can discover, the only really precise thermometric observations by which septic and antiseptic cases under similar conditions can be contrasted and compared are those by Joseph Bell,[1] of Edinburgh, who in

[1] 'Edin. Med. Journ.' for August, 1873.

the same hospital as Lister, follows out, we believe, the antiseptic system.

He has given charts of temperature in twenty-five cases, too small a number to found much on, but he seems to have assured himself of the truth of—and to be on familiar terms with—the following axioms or ideas.

"1. *Suppuration*, even very profuse, does not necessarily imply any great rise in temperature, so long as it is not putrid.

"2. Fœtor or putrefaction of suppuration *always* induces a rise in temperature.

"3. A high temperature, lasting for more than three or four days after the injury or operation, indicates mischief impending, such as sloughing or abscess.

"4. The temperature generally gives warning a day, or even two days, before the pulse.

To illustrate these points, he has divided his cases into three sets:—

"1. Cases of recovery from operation or injury without putrefaction of suppuration 13

"2. Cases where temperature warned of the approach of mischief 8

"3. Cases of fatal issue with high temperature . . . 4

His cases are briefly recorded, but some of these are very remarkable, especially the following:—

"3. *Excision of Breast and Glands.*—J. G., æt. 40. Extensive scirrhus of right mamma and axillary glands. I removed the breast, and dissected out axilla with great care. She made a very rapid recovery; discharge abundant, but quite sweet; and, except the day of the operation, her temperature did not exceed 99·6°, and her pulse kept under 78. This was a remarkable case, from the extent of surface involved.

"7. *Excision of Knee-Joint.*—Mrs. P., æt. 26, had a very bad knee-joint, with abscesses and great destruction of bones. She was losing ground. I excised the knee-joint during a very feverish condition from pain, and the relief was so great that both pulse and temperature fell the very day of the operation. She made a good recovery, and her chart shows a very rapid return to her normal standard, both of temperature and pulse. Discharge in her case was profuse, but antiseptic.

"8. R., æt. 16, was also a case of excision of knee-joint, in which recovery was exceptionally rapid and perfect. I refer to his case only to show how uneventful is the chart of his pulse and temperature. For about five days he suffered a good deal of pain in the leg, after which he could hardly be called a patient at all, as he ate and slept perfectly."

This surgeon also published in the same journal[1] a series of cases illustrative of the antiseptic use of carbolic acid.

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[1] 'Edin. Med. Journ.,' May, 1869.

While in his account of each case there is a want of rigorous scientific accuracy of description, and perhaps a suspicion of youthful enthusiasm, some of the cases are very remarkable, especially two cases of wounds of knee-joint recovering without suppuration, and a case of compound dislocation of the elbow-joint.

In summing up the results, he gives in a few words the expectations he had been led to form from the antiseptic method:

"I trust, however, that the cases I have mentioned will prove to the Society that we are warranted in believing that in the antiseptic principle, explain it as you will, and simplify it as I hope we may, we have a very great addition to our means of combating disease.

Even if on theoretical grounds surgeons may deny the possibility of preventing suppuration, and ignore our facts, still if it be granted that by this method we can diminish the amount and destroy the fœtor of pus, we have done much to improve the sanitary condition, and diminish the fatality, of our great hospitals."

Mr. Annandale, another surgeon in the same school, seems to have faith in antiseptics; and in an interesting paper published in the Edinburgh Medical Journal of January, 1875, he advocates the use of special exploratory incisions with careful antiseptic precautions in cases of doubtful diagnosis. Several classes of such cases are described. Two of early suppuration of hip and knee-joint afford good examples of the boldness and success with which surgeons who have learned to trust antiseptics cut into important joints and obtain cures without suppuration or anchylosis. Readers of this and other similar papers by the surgeons of the northern school who believe in antiseptics will be struck by the confident manner in which they speak of the result of cases *if they can only be kept sweet*, and the freedom with which they cut into joints, expecting that even after such interference mobility of the joint will result.

In Hospital practice we have not only the patient's own welfare to seek and strive for—and we think that some cases nearly prove that a patient whose discharge is sweet runs less risk than if it were putrid or offensive—but we must remember that each patient has an effect for good or evil (generally the latter) on the ward and his neighbours in its beds. Every fœtid ulcer, every putrid amputation or excision, adds its quota of *filth*—call it germs, call it bad air or what you like—to the atmosphere of a room which can hardly hold its own against the respiratory and secretory emanations of twenty sick men, and any mode of treatment which will diminish such fœtor is of incalculable value to the ward and its occupants. In watching practice in Edinburgh we have often seen cases putrid from the

first, patiently dressed with many layers of gauze, which have prevented any odour from the case, and even sometimes by time and patience the once putrid sore has been rendered antiseptic.

Another most important question in reference to the value of antiseptic dressings in hospital practice is, as to the power of preventing erysipelas—pyæmia and hospital gangrene.

On this subject we have many fine vague generalities.

Cumming has no misgivings. He writes:

"A relationship between pyæmia, hospital gangrene, and erysipelas (the dreaded scourges of our large hospitals) may be traced from the fact of their disappearance—coincident with the introduction of antiseptic surgery."[1]

Brave words indeed!

Lister's own views on this subject may be given in his own words:

"And the effects of this rigid antiseptic management upon the hospital atmosphere forms one of the most important features of the treatment. Last evening I learnt from one of the surgeons of a large Liverpool hospital the gratifying news that pyæmia has almost, if not entirely, left wards that were very subject to it before; and this, as far as can be ascertained by the surgeons, from no other cause than the careful carrying out of antiseptic treatment. The results of my own experience in this matter in Glasgow were published nearly two years ago; and I may repeat now what I then said, that wards once among the most unhealthy in the kingdom were converted into models of healthiness, simply as the result of antiseptic treatment. A year ago I published equally satisfactory evidence regarding my practice in the Edinburgh Infirmary for nearly a year. Another year has since elapsed, and during the whole of my Edinburgh period—now almost two years—in wards containing nearly sixty beds, we have not had a solitary case of pyæmia, whilst we are also entirely free from hospital gangrene and from erysipelas. Yet in those wards the beds are placed much closer than is in accordance with modern notions. At first I had them thinned; but learning that patients were placed on "shakedowns" for the night, and finding that, in spite of this arrangement, which of course was the same in effect as if all had beds, the wards remained perfectly healthy, I had the number restored to its original figure. Now I was myself at one time house-surgeon in those same wards for a year and a quarter, and I need hardly say that the surgeon under whose care they used to be was a man under whom things were managed as well as they could be with the means then at a surgeon's disposal—I allude to Mr. Syme; yet I may safely say that such complete immunity from hospital-diseases never existed in those wards before the antiseptic system was introduced.

[1] Op. cit., p. 58.

"Nor has such testimony been borne by myself alone. Professor Saxtorph of Copenhagen, in a letter which I communicated to the 'Lancet' a year since, published most striking information as to a very large hospital previously extremely liable to pyæmia, so that the smallest wounds often gave rise to it, yet remaining for a year absolutely free from the disease, and, so far as he could judge, from no other circumstance than the rigorous adoption of the antiseptic system. Equally satisfactory evidence regarding the healthiness of hospital wards brought about by this means has been given in one of the Blue Books of the navy by Dr. Bernard of the Naval Hospital here."—'Brit. Med. Journ.,' Aug. 26th, 1871.

Berlin.—At the Charité Hospital, Professor Bardeleben uses Lister's treatment carefully and with excellent results. He opens abscesses under spray and attends to all the minutiæ, making free use of drainage tubes and carbolized sponges. An eye-witness reports[1] some interesting cases in which serious wounds healed without suppuration. Langenbeck lately performed an operation in Professor Lister's presence with antiseptic precautions for the first time. It is not often that a man so distinguished and at an advanced age is ready to try new ways. Nussbaum at Munich has found, after the use of antiseptic methods, a great decrease in the amount of pyæmia and hospital gangrene.

On the other hand, we have conflicting opinion as to the power of antiseptic dressings in preventing erysipelas.

Thiersch finds no benefit from its use.

"Erysipelas has occurred sometimes more, sometimes less severely. In 1873, there were seventy-five cases among 1902 patients; in 1874, among 1921 patients the number of cases of erysipelas was distinctly less. In both years antiseptic treatment made no difference. As has been recognised on all sides, antiseptic treatment has no power to prevent the occurrence of erysipelas; it occurs under the use both of carbolic and salicylic acid, sometimes frequently, sometimes rarely, sometimes in a severe and sometimes in a mild form."[2]

Bell[3] records two cases of erysipelas, both fatal, coming on during the progress of antiseptically managed wounds. Our own experience of the gauze as a dressing would rather lead to the conclusion, that in certain skins the constant application of the irritant carbolic acid produces a sort of erythema, which may pass into a distinct attack of erysipelas, in others may cause a species of eczema, which, if the dressing be continued, as remarked above, will pass into impetigo.

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[1] 'Lancet,' June 19th, 1875.
[2] 'Medical Record,' June 2nd, 1875.
[3] Op. cit. 'Edin. Med. Journ.,' August, 1873.

Objections.—1. From the point of view of the Hospital Committee.—It is expensive; at first *very* expensive, and even now certainly it is dearer, that is, surgical dressings now cost much more than they did prior to the coming into use of antiseptics in the Edinburgh Infirmary.

We have obtained the following from a friend in Edinburgh. In the Infirmary there, where only three out of the five surgeons use antiseptic dressings, in 1874 there was used 44,930 yards of gauze at an expense of nearly £600, if the macintosh cloth and oil-silk be also added; while there is no equivalent diminution in the expense for lint bandages, &c.

Well, be it so, this is not a valid or even an important objection if only the results to limb and life are better than those obtained under the old or other systems of treatment. Really good devoted nursing is also much more expensive than the old Mrs. Gamp style; palatial wards, pleasant couches, and high feeding are all expensive; yet no one doubts that the expense is not only justifiable but unavoidable. Perhaps antiseptic dressings will yet justify their expensiveness.

2. From the surgeon's point of view.—Such minutiæ in dressing are tedious, it takes up my time; it is troublesome, and requires to be watched; consequently I must myself dress or watch the dressing of every case. I cannot leave it to house-surgeon or dresser.

This is quite true so far, that certainly each patient's every dressing will take longer time than it does under the old system. Yet, perhaps, in the end time may be saved; for once the case is fairly antiseptic, it need be dressed so seldom—once in three or four days instead of every day; and few will deny that in the end it is for the patient's advantage at least that the surgeon himself should see his wound every time it is dressed; and for the students who visit the hospital, the oftener the surgeon sees his cases the better. We are rather disposed to believe that it is one of the chief advantages of the antiseptic system that it demands a very special individual watchfulness over each patient, if it is to succeed.

But, alas! not only must the patient be watched, but the most unremitting attention must be paid to every action during the period the wound is exposed to view. Is a friendly surgeon going round your ward?—a touch of his finger if unprotected by washing in carbolic acid may ruin the case. Does an instrument fall to the ground or even lie for a second on the table?—to introduce it into the wound again unless dipped is theoretically destruction to your hopes. Is it a warm day, and are you liable to sweat?—a drop falling into the wound or on the dressing

will be fatal to its success.[1] Such being the case, it is not wonderful that in the practice of many who honestly aim at following out antiseptic treatment and believe they succeed, the results have not always been equal to expectation.

Another objection, theoretical perhaps, but still very hard to answer, meets the advocates of antiseptic surgery at the very threshold of their innermost defence. Let us grant, they say, the existence of germs, also that their presence is as dangerous and their absence as delightful as you say it is, how are we to know that these precautions of yours are sufficient to destroy these germs, which are, you tell us, so indestructible? Will one dip of the knife do for the ones on it? Will your spray, fine as it is, suffice to destroy those floating about in the ward atmosphere?

This difficulty cannot be put aside or ignored. Possibly an answer may be found in experiment of Demarquay,[2] who found germs still present, though apparently innocuous, in the serum from perfectly sweet wounds, as if the antiseptic had been enough, without actually destroying the germs, to so far remove any dangerous results from their presence.

Will antiseptic surgery, in its present form, ever make its way into private practice of surgery in the hands of the general practitioner in the towns, or of the country surgeon?

Not to any great extent unless the procedure is very much simplified. The surgeon cannot be expected to lug about with him a steam engine, which requires twenty minutes to get up its steam, or an assistant to work a hand-spray every time he wants to dress an abscess or an ulcer. Nor can a parochial board be expected to furnish antiseptic gauze, protective, macintosh, and bandages out of the rates.

Fortunately in the ordinary routine of country or general practice of surgery antiseptics are not needed. Mother Nature may be trusted to heal most wounds if she is only let alone, and every surgeon who has really seen much good work in private has many a case to tell of, equally remarkable in rapidity of healing, and uneventfulness with those that are vaunted as antiseptic miracles.

Cases there are, however, which, even in the purest air and under the most wholesome conditions, will be treated more safely, rapidly, and fortunately by the adoption in all

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[1] Professor Eberth's, of Zurich, researches on sweat are in this point of view most interesting. He finds in the axilla, breast and inner side of thigh of persons in a state of perspiration, an enormous number of bacteria. In most cases they originate from minute bodies found upon the hairs in these regions. They rapidly increase in number and are nearly indifferent to reagents—concentrated acids, alkalies, ether and chloroform!!! In 'Virchow's Archiv' for February, 1875.

[2] 'Gazette Medicale de Paris,' August 29th, 1874.

its strictness of antiseptic treatment. Chief among those are all abscesses connected with or dependent on the presence of diseased bone, lumbar, psoas, joint, &c., all cases of compound fracture except the very simplest, and some cases of ligature of great vessels.

With regard to these last, however, those who advocate antiseptics will find it difficult to answer those who may say to them, Do not preach on the blessings of antiseptics till you can show us a series, like Syme's, of thirty-seven cases of ligature of the femoral for aneurism without a single death.

Great men lived before Agammemnon, and we all have read in the classics of surgery of statistics of amputations, which will be hard to beat. Still progress ought to be our aim, and a science which stands still is going back.

In closing this brief, imperfect, and (from the very nature of the subject and state of the question) unsatisfactory notice of antiseptic surgery, we must say that, in common with many others who have visited Edinburgh to watch the system in operation, we have been deeply impressed by the zeal and simplicity, the patience and scientific method, brought to bear on the subject by its inventor.

II.—Recent Researches in Nerve Pathology.[1]

Paralysis Agitans and Insular Sclerosis.—The two diseases which are here considered together have very close affinities in many important particulars. Alike in the obscurity of their causation, alike in their morbid anatomy, alike in their clinical features, their resemblance is still further increased by the slowness with which each has gained a footing amongst recognised diseases in this country.

Although paralysis agitans was described as long ago as 1817 by our countryman Parkinson, and although it has been elaborately treated of by Dr. Sanders in 'Reynolds' System,' the number of complete cases published in England may be counted on the fingers of one hand; and as to insular sclerosis, so little attention has it attracted that a learned French author can

[1] 1. *Leçons sur les Maladies du Système Nerveux faites à la Salpêtrière.* Par J. M. Charcot, Professeur à la Faculté de Médecine de Paris, &c., &c.
2. *Eight Cases of Insular Sclerosis of the Brain and Spinal Cord.* By W. Moxon, M.D., 'Guy's Hospital Reports,' vol. xx, 1875.
3. *On the Morbid Histology of the Spinal Cord.* By W. B. Kesteven, F.R.C.S., 'St. Bartholomew's Hospital Reports,' vol. viii, 1872.
4. *Miliary Sclerosis; its Pathological Significance.* By W. B. Kesteven, F.R.C.S., 'British and Foreign Medico-Chirurgical Review,' July, 1874.

express a doubt as to whether the disease is even known on this side of the Channel.

The most complete description of these maladies is contained in Prof. Charcot's admirable 'Lectures on the Diseases of the Nervous System;' a work which we commend to our readers as containing on many subjects, and especially on the diseases of the spinal cord, the most recent, exact and comprehensive knowledge obtainable, related with all the national precision of idea and of diction, and with an infusion of interest, sometimes amounting to enthusiasm, which is peculiar to the author. So accurate have become the observations in the clinic, and so definite the morbid changes discovered by new methods of examination, that Prof. Charcot is able to map out the cord into pathological regions and to say with precision what symptoms shall follow from disease of each one, and, conversely, what regions shall be found diseased when such and such symptoms are presented. When it is remembered how short a time it is since locomotor ataxy was definitely located as disease of the posterior columns, and when it is considered that a standard text-book still in use can describe multitudinous nervous disorders, amongst others chorea and epilepsy, as symptoms of a disease called "spinal irritation," it will be seen how vast is the advance which these lessons indicate.

Doubtless Prof. Charcot's work is not free from the defect, inseparable from the method pursued, of sometimes drawing a sharp distinction between things which in nature merge into one another, thus rendering it difficult for others to obtain the same definite results that he does. To instances of this kind, we shall subsequently direct the reader's attention; but so slight a drawback may be readily pardoned in view of his very successful attempt to reduce the chaos of spinal disorders into some definite order.

Dr. Moxon's report of the eight cases under his care is the only account of insular sclerosis in the language, and his cases were the first in this country in which the disease was diagnosed and the diagnosis verified. His readers are greatly indebted to him for the acumen with which he has separated the essential symptoms from the non-essential, and presented a most concise description of the usual features.

Mr. Kesteven is well known for his laborious and very valuable researches into the morbid histology of the spinal cord; and his name is especially associated with a change known as miliary sclerosis, which must be carefully distinguished from that called insular sclerosis, which we are about to consider. The one is a change invariably associated with a definite group of symptoms; the other, although a gross and destructive

change, is common to a great number of diseases most diverse in their manifestations.

It will, perhaps, give the reader the best warrant of the completeness of the lectures, and at the same time the most vivid idea of the diseases, if we relate an imaginary case of each compiled from Prof. Charcot's account, checking it where desirable by comparison with the other works.

Taking first paralysis agitans, we find that the patient may be of either sex, and of any profession or station in life; he is more than forty years old; he has, perhaps, lived in a cold damp place, and has suffered much mental distress. For some time before he comes under care he has suffered from a sense of great fatigue, or it may be from rheumatic or neuralgic pains in the limb which is about to be attacked by tremor. Gradually the agitation from which the disorder takes its name begins to affect, say, the forefinger and thumb of the right hand. These digits are perpetually quivering and striking together as if the patient were taking pinches of snuff or rolling pellets of paper. Little by little the tremor affects the other fingers, then the hand; and to this it may remain restricted for a long while, even for years. Eventually, however, it spreads up the right arm, and at the same time the right foot becomes involved; and after a while the left hand and left foot follow the same course.

The muscles of the face do not become tremulous. On the contrary, they become unusually immobile, and give the face a remarkable fixed, sad, dogged expression, which of itself is almost diagnostic of the disease. This fixity of the face is but part of a general rigidity which at a later period affects the muscles of the neck, trunk, and some at least of those of the extremities. The effect of this rigidity is to impose on the patient a most remarkable attitude and gait. The head is strongly inclined forwards, and the body also leans in this direction; the elbows are a little away from the sides, the forearms flexed on the arms, the hands flexed at the wrists and resting on the waist, while the fingers maintain their constant oscillations. If the patient be told to walk, he rises slowly and with difficulty from his seat, hesitates for some seconds, and then starts into a rapid shuffling trot, "as if," says Prof. Charcot, "he were running after his centre of gravity, which still escaped him." Or, instead of this, he may have a tendency to go backwards.

It is to be remarked that the patient has no nystagmus, nor is his articulation affected as it is in Sclerosis, unless indeed the agitation of the body be so great that it communicates an interruption to the voice like that which affects a novice in equitation when his horse begins to trot. As a rule, however, the voice,

far from being tremulous, has a monotony of expression similar to that of the face. The patient speaks abruptly in monosyllables or short sentences, and uses no cadence, but maintains his voice at one invariable pitch from the beginning to the end of his sentence, a peculiarity which renders his tone very striking.

Besides these motor troubles, the patient has affections of sensation, which, though they cannot be called painful, are sometimes sufficiently severe to render his life quite burdensome. There is a constant feeling of tension in the muscles, a sense of fatigue, and an indefinable uneasiness which urges him to incessantly change his position. Besides this, there is a feeling of excessive heat which compels him even in the depth of winter to toss off all his clothes. In spite of this, however, the temperature is not raised, and this is in accordance with the rule which Prof. Charcot and Dr. Bouchard have enunciated, that in static contraction of muscles, or that contraction which is chiefly tonic, the temperature is raised, while in dynamic contractions, or those which are chiefly clonic, the temperature remains normal. This is what we should *à priori* be led to expect, since in the former case the work done is wholly out of proportion to the vigour of the contraction, and we should therefore expect a production of heat in lieu of the arrested mechanical motion, while in the latter case, since the limbs are moved, work is done, and, the greater part of the force being expended in mechanical motion, there is but little to be converted into heat.

We have related these symptoms as occurring continuously, but the reader must understand that they are spread over a very long time, ten, twenty, or even thirty years elapsing before the terminal period arrives. Sooner or later, however, the motor trouble increases to such an extent that the patient is obliged to remain in his chair all day, and at last is confined to bed. Then his nutrition suffers, his intellect becomes obscured, he gets more and more prostrate, at last bed-sores appear on his sacrum, and he dies from gradual exhaustion; the characteristic tremor having much diminished and perhaps altogether ceased during this terminal period.

Such is the general course of a typical case of paralysis agitans, but of course there are many variations in the severity of the different symptoms, and it is comparatively rare to meet with a case which presents them quite like the foregoing. Prof. Charcot states that there are rare cases in which the muscular rigidity is one of the earliest symptoms, so that before the tremor has appeared, or when it is but very slight, the attitude and gait are already very pronounced. We believe that it is

not uncommon for the fixed expressionless face, the peculiar voice and the characteristic attitude to exist early in the disease, when tremor must be narrowly looked for to be detected at all. The agitation is not, as the nomenclature implies, the most characteristic symptom. The termination, too, is more often brought about by some intercurrent affection, by pneumonia for instance, than by the course of the disease itself.

There is a certain satiric humour in Prof. Charcot's notice of the morbid anatomy of paralysis agitans. He divides the autopsies hitherto made into three groups. In the first group nothing at all was found. The second group comprises cases of supposed paralysis agitans, which Prof. Charcot considers were in reality sclerosis; and the third group contains the case of Parkinson subsequently mentioned, and a similar case by Oppolzer which is treated with similar distrust. There are, however, other cases on record which give much more satisfactory results. Leyden has reported one in which the agitation was limited to the right arm, and a sarcoma the size of a large nut was found in the optic thalamus of the opposite side. Murchison and Cayley have reported a case in which very definite changes, partly of sclerosis and partly of cell growth, were found in the cord; but as in this case the symptoms are described but very briefly, it is possible that Prof. Charcot would place it in his second group. Joffroy, however, took especial care to investigate this point, as to whether the cases were really paralysis agitans or insular sclerosis, and he states that two out of his three cases were clearly paralysis agitans. In these two there was exuberant growth of the epithelium of the central canal and of the nuclei around. In the third case, which seems not to have been a very doubtful one, there was in addition a sclerosed patch in the medulla.

In the brief historical introduction to insular sclerosis Prof. Charcot notices the total absence of all mention of this disease from the standard works of this country, and says that even to this day he does not believe we have heard of it. It is impossible not to admit the justice of the implied reproach, although had Prof. Charcot paid a visit to the wards of our hospitals he would have found that the disease is by no means so unknown as its absence from our literature would seem to indicate. Nevertheless, it must, as Dr. Moxon says,

"appear strange that the singular and definite disease . . . has not yet been admitted by the profession in this country, when we know that it not only has constant and characteristic symptoms, but also a quite peculiar and very remarkable morbid anatomy."

When it is considered that in insular sclerosis there is a gross

change which may affect any part or any combination of parts of the cerebro-spinal axis, it will be readily understood that the symptoms to which this change may give rise will differ very widely with the part of the nervous centre involved; and when it happens to involve a part which is a common seat of other changes, the symptoms of sclerosis will closely simulate those of other diseases. Àpropos of this peculiarity Prof. Charcot relates how a distinguished physician. who, however, was not familiar with the symptoms of sclerosis, paid a visit to the hospital where it abounded. Prof. Charcot's colleague showed him a case of the new disease; it was a beautiful specimen of the cerebro-spinal form. The patient left his bed and made a few steps down the ward. "He is an ataxic," said the visitor. "Perhaps," replied the host; "but what do you think of these rhythmic movements with which the head and upper extremities are affected?" "I see," replied the visitor, "he has chorea besides, or perhaps paralysis agitans." The patient was then interrogated, and replied with a peculiar defect of articulation, and often with slight tremor of the lips. "I find," said the physician, "you wish to puzzle me by showing me a case of extreme complexity. Here are now the symptoms of general paralysis. We will go no further; your patient unites, perhaps, in himself the whole pathology of the nervous system."

In spite of this, however, it is by no means difficult, at any rate in an advanced case, to diagnose the disease, which we shall now proceed to describe in the same manner as the last.

When the malady first appears the patient is a young adult. The first symptom noticed by her (for it is more common, according to Prof. Charcot, in women than in men) is a gradually increasing enfeeblement of the lower limbs—a symptom common enough, but which excites suspicion of sclerosis if there is no accompanying disturbance of sensibility, no atrophy of the muscles, no bladder or rectum trouble, and particularly if there are intermissions in its course. This may remain the sole symptom for months or even for years before the advent of the characteristic tremor places the diagnosis beyond a doubt.

The tremor of insular sclerosis is quite peculiar, and by the exercise of ordinary care may be easily distinguished from other forms which at first sight resemble it closely. When the patient is in the recumbent posture, abandoned to complete repose, there is no trace of tremor in any part of the body. When she is seated there is no tremor in the extremities, which are at rest, but the head and trunk, which are sustained by muscular action, are agitated by a slight trembling motion. Now, if she be told to carry a glass of water to her mouth, immediately the arm begins to move tremor is developed, and the more prolonged

and extensive the movement the greater become the excursions of the tremor. When first she grasps the glass the tremor is scarcely noticeable, but as she continues the movement the agitation becomes more and more severe until when it reaches the mouth the glass is clattered against the teeth and the water is spilt. Similarly, when first she begins to walk the tremor does not interfere greatly with her progress, but at each step she staggers more and more until it is necessary to support her in order to save her from falling. Let her now be placed on her bed and immediately all tremor ceases.

The tremor of paralysis agitans, with which, no doubt, insular sclerosis has often been confounded, unites with great general similarity several points of marked difference.

In the first place, the tremor of paralysis agitans exists just as much during repose as during exertion, and never departs except under the influence of slumber. Dr. Moxon says that "in paralysis agitans the patient is much less disturbed by it [the tremor], and there is not that extreme, almost idiotic helplessness of manner which characterizes sclerosis, due doubtless to the extensive destruction of brain," &c. It is evident that this applies to those cases only in which the brain is involved as well as the cord. Again, while the head, and particularly the lips, are greatly agitated in sclerosis, in paralysis agitans they are remarkably fixed. Then, too, in the latter affection the shocks of the tremor have less excursion, are more regular, more rapid, more crowded together, as it were, than they are in sclerosis, in which the oscillations are more ample and more approach the gesticulations of chorea.

It is always easy, however, to distinguish the disorderly and *bizarre* movements of chorea from the rhythmic oscillations of sclerosis ; indeed, Dr. Moxon says that the latter are not in the least like chorea, and that he will not waste time in giving differences. It is only necessary to notice that in sclerosis the general direction of a movement, as for instance that of carrying the hand to the mouth, persists in spite of the tremor, while in chorea the act is interefered with by absolutely contradictory movements.

Then also in locomotor ataxy, although there are abrupt disorderly gestures, there is no true tremor, no rhythmic succession of shocks, as there is in sclerosis ; and in the latter malady closure of the eyes has no effect on the movements.

In mercurial poisoning tremor occurs which is wholly indistinguishable from that of sclerosis, but in which, according to Dr. Moxon, there is not the *embarrassed helplessness* of sclerosis.

This gradual enfeeblement with the accompanying tremor, the

most striking as it is the most characteristic symptom of sclerosis, spreads at length to the upper extremities; and when these the spinal symptoms are well developed, a new set of symptoms dependent on affection of the intracranial ganglia become superadded to them. This group of symptoms comprises certain affections of the eyes, of the articulation, and of the intelligence.

The patient may have diplopia, but it will be only a passing symptom; and she will be likely to have amblyopia with or without optic atrophy; but the most remarkable symptom which appears about this period is the peculiar movement called nystagmus—a rapid simultaneous oscillation of the two globes from right to left and from left to right, sometimes disappearing when the eyes are at rest, but instantly called into play by fixing the gaze on an object.

The defect of articulation is very like that of general paralysis; the same tremor of the lips, the same drawling utterance, the syllables being prolonged and broken into a bleat, and the same difficulty with certain consonants, particularly *b*, *p*, and *g*.

The defect of intelligence is of the nature more of a general enfeeblement of mind than of any special defect. The patient laughs and cries with unusual facility, is hopeful or depressed without reason, and loses to a considerable extent her memory, but there is seldom any actual alienation. Dr. Moxon says of one of his patients—

"Her intellect was narrowed without unclearness, so that on the daily course of things before her eyes she replied tolerably well, but could not go much beyond this. There was no delusion, and her conduct and demeanor were always as right as the . . . disabilities allowed."

The complete development of the foregoing symptoms indicates the approaching termination of the first stage of the malady—a stage which dates from the first appearance of the symptoms to the time when the tremulous mobility of the limbs gives place to a spasmodic rigidity, and which may last for two, four, or even six years.

The rigidity characteristic of the second stage begins by slight isolated attacks, which gradually increase in length and severity until the members are reduced to a condition of permanent immobility by the spasmodic contraction of their muscles. At this period the lower limbs take the following position:—The thighs are extended on the pelvis, the legs on the thighs, and the feet are in the position of talipes varo-equinus. The knees are generally so closely applied to one another that they can only be separated by using considerable force. If the extremity of one foot be taken in the hand and briskly extended, the whole

of the corresponding member is seized with a convulsive tremor which reminds us of that produced by strychnia poisoning. This tremor, which may even extend to the opposite limb, and which will continue for several minutes if left alone, may be immediately arrested by sharply and forcibly flexing one of the great toes. The same tremor may be caused by various kinds of irritation, and sometimes develops spontaneously or on attempts at voluntary movement. The complete accuracy of Prof. Charcot's description will be at once acknowledged by those who have clinically studied this curious phenomenon, which, as Prof. Charcot observes, is identical with the spinal epilepsy of Brown-Séquard. It is, however, remarkable that with such complete loss of power, dependent on a spinal lesion, there is no diminition of electric irritability in the affected muscles. Before the spasmodic rigidity appears the irritability is certainly normal, and after the accession of the rigidity the reflex excitibility is so great as to render observation exceedingly difficult.

There is one other symptom which the patient is most likely to exhibit at this stage of the malady; this is the occurrence of epileptiform or apoplectiform attacks, further developments, no doubt, of the vertigo which occurs as one of the earlier symptoms. These attacks may be slight or they may be very severe, even so as to be the immediate cause of death.

With all this motor disturbance there is no anæsthesia, no loss of cutaneous sensibility; and Dr. Moxon remarks that the rule in all organic spinal palsies is that the motor power is lost much more quickly, certainly, and entirely than the sensory power. On this point, indeed, all observers are agreed.

After a period of several months or years the commencement of the third stage is indicated by a general impairment of the vital function. Habitual want of appetite and diarrhœa bring on progressive emaciation; the sphincters lose their power; the mucous membrane of the bladder becomes inflamed and ulcerated; bedsores appear on the sacrum and on every point exposed to pressure, and rapidly increase until they attain enormous dimensions. At the same time the original symptoms become exaggerated, the obscuration of the intellect increases to actual dementia, and the defect of articulation becomes such that the patient can only express himself by an unintelligible growl. When matters have arrived at this pitch death is not far off, and may be brought about by the continuance of the foregoing symptoms or by some intercurrent pulmonary or intestinal affection.

Such is the course of a complete case of disseminated cerebro-

spinal sclerosis, but the reader will understand, as before, that these symptoms may be met with in very various combinations, according to the parts of the central nervous system involved in the disease. Sometimes the disease is limited to the cord, and the symptoms are correspondingly incomplete; in other cases the brain only is involved, and then the converse obtains; yet a third class of cases have in addition an unusually complete involvement of the posterior columns, and then the symptoms of locomotor ataxy will be superadded to those of sclerosis. There is a fourth variety which, although very uncommon, is sufficiently important to deserve separate mention. In some cases the patients are suddenly seized with the symptoms of *bulbar paralysis,* better known in this country as glosso-labio-laryngeal paralysis, to which they rapidly succumb, the autopsy displaying the cause of death in a recent patch of sclerosis in the medulla oblongata.

The main symptom displayed by the two diseases here considered, the symptom which groups them together and separates them widely from almost all other diseases, in which are to be found their closest resemblance and their clearest difference, is tremor. This symptom has at present attracted little attention, and remains wholly unexplained except by such solutions as are offered by Jaccoud, who says—" L'affaiblissement de l'innervation de stabilité est la condition pathogénique du tremblement;" and Charcot, who says—" The transmission of voluntary impulses still takes place through the denuded axis-cylinders, but it takes place in an irregular, jerky fashion, and thus is produced the tremor which embarrasses the execution of voluntary movements." Tremor appears to us a phenomenon of such importance, as well on account of its physiological bearings as from its diagnostic value, that we propose to examine it in some detail.

In a discussion on " tremor in general" which introduces the chapters on paralysis agitans and insular sclerosis Prof. Charcot insists that a sharp distinction should be drawn betwixt two varieties of tremor. The first variety affects the patients only when they perform voluntary movements; while they are at rest their limbs are perfectly still. The second variety is continuous. Whether they are in action or in repose, whether sitting, standing, or lying, the limbs of these patients are agitated by a ceaseless trembling which leaves them only on the advent of sleep. Prof. Charcot claims, not only that this is a most important clinical division, which it undoubtedly is, but even that the two varieties are physiologically distinct, and he quotes with approbation Van Swieten's classification into *tremor coactus* and *tremor a debilitate.* Although it is with

diffidence that we place ourselves at variance with Prof. Charcot on a subject which he has made so peculiarly his own, it appears to us that in this instance he has allowed his desire for extreme clearness to induce him to make a separation in kind where in nature there is only a difference in degree.

The continuous action of the muscles in health, the unsteadiness of fatigue, the intermittent tremor of sclerosis, the perpetual agitation of paralysis agitans, and the rhythmic action of the heart, appear to us only degrees of the same phenomenon.

The sustained contraction of a voluntary muscle has been shown by well-known experiments to be the result of innumerable separate contractions of the muscle repeated at extremely small intervals of time, thus becoming confluent and producing a uniform effect; in the same way that, if the edge of a card be pressed against a revolving cog-wheel, we hear, not the separate taps of the cogs against the card, but a continuous musical note. If the wheel be made to revolve more and more slowly, the pitch of the note becomes lower and lower until at last the sounds become discrete and we distinctly hear each separate tap as each cog strikes the card. It is the same with the muscular contraction. If the interval of time between each two successive shocks become greater and greater, the relaxation which takes place after each one has time to become more and more complete, until the continuous contraction becomes first remittent and then intermittent. Hence the sole difference between the rhythmic action of the heart and the continuous action of the voluntary muscles is that in the former the long interval between each shock and its successor allows of complete relaxation, while in the latter no sooner has the relaxation begun than a new shock catches the muscle and recontracts it before the relaxation is considerable enough to be appreciated except by the most delicate instruments. Similarly, the difference between the same continuous action and the remittent action in sclerosis is that the minute interval of the former becomes in the latter great enough to be appreciated by the unaided senses.

The question of course arises, what is the cause of this lengthening of the interval? which involves the question, why is there an intermittent action at all—why is it not continuous? The answer is easy. Not, only as in the foregoing cases, may an intermittent cause produce a continuous effect, but conversely a continuous cause may produce under certain circumstances an intermittent effect. The most obvious illustration is supplied by physics. When the disc of an electrical machine is turned, electric tension increases continuously on the prime conductor until it exceeds the resistance of the air, and then a

sudden discharge takes place, which is repeated again and again at successive intervals whenever the tension is brought up to the proper pitch, the rate of production of the electricity remaining constant. Here, then, is an intermittent effect produced by the continuous accumulation of a force opposed to a constant resistance. More than this, it is found that by increasing the resistance, say by lengthening the distance the spark has to travel, the tension necessary to overcome the resistance is increased, and therefore also the time during which the tension must accumulate, in other words the interval between two successive shocks. Similarly, by diminishing the resistance we may render the shocks more rapid. As a corollary from this we have the necessary result that the longer the interval between two successive shocks, that is, the slower the shocks, the more powerful are they when they do occur. Since it seems to be an established doctrine that the function of nerve-cells is to store up and expend force, the application of these principles to nerve-muscle phenomena seems quite allowable. The discharge of force from the nervous centre to the cardiac muscles takes place at considerable intervals of time; the effect of each discharge is therefore great, and we have no doubt the reason is that the resistance is great also. The normal discharge to the voluntary muscles takes place, we suppose, under extremely low resistance; the discharges therefore follow one another with extreme rapidity, and the effect of each one is exceedingly small, the visible effect being that of the aggregate of a great number.

Now let us suppose that this resistance is increased, by which, of course, we mean the resistance to be overcome by the nerve-current descending from the nerve-centre to the muscles, resistance analogous to that expressed by (R) in Ohm's law. The necessary consequence will be that the discharges will be slower and stronger; and instead of the fine remission of normal contraction is produced a coarser remission which breaks the continuity of the normal action into a fine tremor. If, therefore, we can show that in paralysis agitans and sclerosis the resistance to the passage of the current along the nerve-fibres is increased, the tremor is completely explained on purely physical grounds. In the description of the minute anatomy we shall show that the nerve-fibres are deprived of their insulating material, that they are strangled by the contraction of a fibroid growth, by which they are warped and wrung out of their normal positions, and that their diameter is very greatly diminished. There is no doubt that these conditions do materially diminish the conducting power of the fibres; that is, they increase the resistance.

Whether the tremor of fatigue be due to increased resistance or to diminished rapidity of accumulation in the centre is not material to the argument, but the fact that fatigue does cause tremor in healthy subjects, tremor which first shows itself on movement only, and, if the fatigue be greater, at rest also, proves that the two varieties of tremor are not radically distinct.

The description of the morbid anatomy of sclerosis shows that Prof. Charcot carries into the post-mortem room and the laboratory the laborious observation and the same minute attention to accuracy which render him so excellent a guide in the clinic. Taking first the macroscopic appearances, he describes the morbid change as consisting in a number of grey patches, irregular in shape but of definite and clearly marked outline, scattered over the whole length of the cord and extending up to the medulla, the pons, the cerebellum, even invading parts of the cerebrum itself and the central ends of cerebral and spinal nerves. On making a section across one of these patches it is found that they extend to variable depths and in the most irregular manner into the substance of brain and cord, so that the surface markings afford no reliable criterion of the extent of tissue affected. The patches have a firm consistence, a clean section, and their colour is a rosy grey.

When a low power of the microscope is brought to bear on a thin section of the cord it is found that the sharp separation between the healthy and the diseased parts is only apparent, and that really they merge into one another by insensible gradations. Directing our attention first to the healthy portion of a transverse section, Prof. Charcot describes the well-known appearance of the white substance, the numerous discs formed by the cut ends of nerve-fibres, each with its central axis-cylinder and its sheath of myeline. Between the fibres appears the much-discussed connective substance, neuroglia or reticulum, to which, as the seat of the principal and probably the primary change in sclerosis, Prof. Charcot devotes great attention. He adopts the view that the neuroglia is a true lymphoid connective tissue, composed of nucleated stellate cells, by the union of whose processes the reticulum is formed. Although, however, he states some of the objections to this view, he does not mention those which appear to be the most powerful—the argument of Robin that, whereas all the other connective tissue is developed from the mesoplast, the nervous centres are developed wholly from the epiplast; and the observations of Henle and Meckel that the chemical reactions of the neuroglia are the inverse of those of ordinary connective tissue. However, as Jaccoud truly says, the difference is merely whether we term the morbid change a

hyperplasia of a tissue pre-existing or a neoplasia, and the difference makes not the slightest alteration in the clinical significance of the change.

Passing from the normal cord into the peripheral zone of a patch of sclerosis, it is noticed that the trabeculæ of the reticulum are notably thickened, having sometimes double their normal diameter. At the same time the nuclei which occupy the intersections of the reticulum have increased in size and number. Jaccoud, following Rindfleisch and Frommann, lays great stress on the accumulation of the nuclei, which he represents as being very abundant, but Prof. Charcot relegates this change into a subordinate position. The nerve-fibres appear more distant from one another; in reality they are diminished in volume, the decrease being due solely to loss of the myeline sheath, the axis-cylinder remaining normal or becoming slightly hypertrophied.

Taking a part nearer to the centre of the patch (zone of transition), the nerve-fibres are seen to be still further diminished. Many of them seem to have disappeared; in reality they have been deprived of their sheath of myeline, and are represented only by their axis-cylinders, which have acquired, according to Prof. Charcot, "colossal" dimensions. The trabeculæ of the reticulum have become more transparent, their outlines are less definite, and in certain parts they are replaced by long and fine fibrillæ like those of ordinary connective tissue.

In the central region of a sclerotic patch the change has become extreme. The fibrillated connective tissue is now very greatly developed at the expense of all the other elements. The fibroid reticulum has disappeared; the nuclei have diminished in size and number; the myeline has vanished. Still a certain number of axis-cylinders remain intermixed with the fibrillæ, but they have no longer the dimensions that they had in an earlier stage; most of them, indeed, are diminished in size so that they can with difficulty be distinguished from the connective-tissue fibrils. The persistence of a certain number of axis-cylinders is a constant occurrence in insular sclerosis, but probably the majority undergo the change, described by Frommann, of first showing a longitudinal striation, these striæ then breaking up into granules which disappear by absorption. Besides these changes in the normal constituents of the cord, there appears another element which is not found except in disease. Among the nerve-fibres are found bodies of irregular rounded outline, having the double contour and the other physical properties of myeline. Existing in the peripheral zone in large isolated masses, they become smaller and more numerous as the change advances, until they are at last represented by

minute granules which have all the appearance of fat. These granules are not found in the centre of the patch, but only in the peripheral and transition zones; and, as they are there found in great abundance around the vessels and in their lymphatic sheaths, it is probable that they are thus absorbed.

Having studied the successive appearances presented by a patch of sclerosis when examined from the periphery towards the centre, that is, from the most recent changes to the most advanced, we are in a condition to consider what is the sequence of the appearances we have witnessed, which are primary, and which are merely subsidiary. On this head Prof. Charcot has no doubt. Incontestably, he says, the multiplication of the nuclei and the hyperplasia of the reticulate fibres of the neuroglia are the initial fundamental fact, the necessary antecedent. The degenerative atrophy of the nervous elements is secondary. Dr. Moxon, too, says that the minute anatomy is evidently that of chronic inflammation, and describes it as beginning in the medullary rays of the connective tissue, involving the nerve-fibres secondarily. Jaccoud also speaks of sclerosis as constituted by two acts of the connective tissue—first abnormal development or proliferation, and then contraction and compression. Rindfleisch is of the same opinion. So that there can be little doubt that sclerosis of cord is strictly analogous to those changes which have so long been known to take place in other organs, in which the same sequence of proliferation and contraction of connective tissue leads to the same disastrous effects on the structure and function of the organ.

The cause of the peculiar distribution of the patches of sclerosis is at present unknown. Prof. Charcot does not hazard any hypothesis. He states that Rindfleisch has brought forward the notion that the point of departure is in the vascular system—that inflammation of the walls of the small vessels is the first change, and that from these, as from centres, it is propagated. Rindfleisch, however, clearly distinguishes two kinds of sclerosis. In one form there is invariably a thickened vessel in the centre of the patch; in the second, besides other differences, he distinctly states that the behaviour of the vessels is of subordinate significance. The fact that he limits the term sclerosis to the first variety, and calls the other grey degeneration, has probably given rise to Prof. Charcot's error. A second hypothesis brought forward by Dr. Moxon deserves attentive consideration, both from its novelty and from the connection it would establish between disorders at first sight widely different. The supposition is that there is reason to regard insular sclerosis as *eruptive*—to look upon the phenomenon of eruption as by no means confined to the skin, but producing characteristic ap-

pearances in many other organs. In smallpox the mucous membranes, the inward reflexions of the tegument, show the characteristic eruptions. In syphilis the same kind of action is found in muscles, bones, &c., in the form of gummata. But the closest resemblance to sclerosis is found in the subacute arteritis occurring in patches, which Dr. Moxon has described as *inflammatory mollities.* The fibroid substances in brain and artery resemble each other very closely, and the form of distribution of the charge—round blotches—is the same in both. In either case we meet with a disease of unknown origin, which presents itself as an eruptive outburst of more or less circular foci of subinflammatory action. In neither case is there any correspondence of the figure of the patch with any structural component of the texture. Dr. Moxon believes that both maladies have an essentially eruptive nature, by which he means that the local disease is set up by some agency which is of specific kind and not native in the part in which the change is seen. It is to be hoped that Dr. Moxon will further elaborate an idea of such fruitful promise—an idea which, if really established, will give us a far clearer insight into a class of obscure diseases, and furnish very valuable indications for preventive and curative treatment.

It remains now to consider what is the connection of the symptoms of the disease with the peculiar change in the nervous centres, and here, again, the deficiency of our knowledge is conspicuous. The uniform coexistence of the morbid change with the peculiar group of symptoms described leaves no more doubt of its causal relation to them than of the causal relation of sclerosis of the posterior columns to the staggering gait and electric pains of locomotive ataxy; and this is further confirmed by the fact that in other diseases (cerebral change with descending wasting, pachymeningitis, primitive symmetrical sclerosis, &c.) damaging the antero-lateral columns there occur paralysis and spasmodic rigidity. But in what way it is that change in this region gives rise to these symptoms is at present unexplained. We have endeavoured to show that tremor may be rationally accounted for by the existence of such a condition in the nerve-centres, but to discuss the dependence of the other symptoms on the anatomical condition would, in the present state of our knowledge of the functions of the different regions of the cord, merely lead us into idle speculation.

Our knowledge of the etiology of these, as of most other nervous diseases, is still in a very unsatisfactory condition. It is indeed gratifying that the three things which have so long been credited with the parentage of every kind of nervous disorder—syphilis, masturbation and sexual excess—seem at

length to be losing their time-honoured reputation. None of these is mentioned by Prof. Charcot. The imaginations of pathologists on this subject have been much like that of the celebrated sailor to whom was granted the fairy privilege of having three wishes fulfilled. After he had secured all the rum in the world and all the tobacco in the world by his first two wishes, he could think of nothing further to wish for than a little more rum. After crediting all nervous disorders to sexual excess and syphilis, pathologists have no explanation of a new disease but a little more sexual excess.

It is interesting to note the contrary results to which two excellent observers are led by reasoning from insufficient data. Jaccoud finds that sclerosis attacks young or adult individuals without distinction of sex up to the age of forty-five years, and he has known two cases over fifty. Prof. Charcot considers it established that the malady is much more common in females than in males—that it rarely appears after thirty, and never after forty. Jaccoud finds that of eighteen cases ten are male and eight female. Prof. Charcot, out of thirty-four cases, finds nine male and twenty-five female. Both observers agree, however, in recognising the powerful influence of moist cold in causing both paralysis agitans and insular sclerosis, and the latter adds vivid emotions as another fruitful cause. In this, as in all similar cases where such very common conditions are arraigned as causes of such rare diseases, it is evident that they cannot be the sole nor even the principal causal conditions, or the diseases would be far more common. If, as Prof. Charcot states, the anxiety caused, for example, by an illicit pregnancy can give rise to sclerosis, the disease would scarcely have remained so long unrecognised on account of its rarity.

Much more importance must be attached to a fact noticed by Dr. Bourneville, that the appearance of the symptoms of sclerosis is often preceded by some acute illness. It is possible that a damaged cord, which was able to perform its functions without great defect so long as the general health was good, gave way suddenly when the vital condition was lowered. This, at least, was suggested as being the case in a patient whom we recently saw with an able pathologist and physician. A woman had centripetal paralysis of all four extremities with muscular atrophy, and she dated these symptoms from her confinement two months before. The view of the case, founded on a general examination of her system, was that she had had disease in her cord for a very long period before the symptoms showed themselves, and that the confinement had brought them out in some such way as this. Suppose a fibroid thickening of the vessels of the cord such as, judging from her clinical history, there was no doubt

she had in other organs, so long as the heart retained its full power, the *vis a tergo* would be competent to force the plasma through the thickened walls of the vessels in sufficient quantity to sustain the nourishment of the cord nearly up to the normal standard; but when the heart's power was suddenly much diminished the cord would be very insufficiently nourished, and its function would rapidly fail. In view of these circumstances a very favorable prognosis was given, it being believed that as the heart regained power the cord would regain its function; and this was fully justified by the event. The patient recovered. The case, however, is not decisive, for she took iodide of potassium.

Keeping in mind the profound alteration of structure on which the symptoms undoubtedly depend, we should anticipate that drugs would be useless in the treatment of this disease. What drug could restore the lost myeline to the nerves, or reconvert the dense fibrillar connective tissue into cobweb-like neuroglia? We are prepared to hear, therefore, that the whole catalogue of so-called nervines—chloride of gold, phosphate of zinc, nitrate of silver, bromide of potassium, arsenic, belladonna, strychnine, ergot of rye, and the rest—have one and all failed to benefit the patients, although several of them have appeared to exaggerate the symptoms. Rather, we should imagine, by careful abstinence from fatigue and anxiety, by rigorous dietetic and hygienic rules, by the removal of any concomitant maladies or sources of weakness, should the general standard of nutrition be raised to the highest possible pitch; so that although we cannot renew the damaged patches in the cord, yet by ensuring them a copious supply of pure, rich, well-oxygenated blood sent by a powerful heart, we place those elements that yet remain under the most favorable conditions possible for retaining and improving their function.

III.—Paget's Clinical Lectures and Essays.[1]

Some of the most useful teaching of men of large experience and wide scope of mind is that which may be gathered from the casual reflections and remarks which are called forth by particular subjects under investigation. Herein the teacher is not hampered by the necessity of reducing his thoughts to a systematic form; he can throw out hints and state impressions

[1] *Clinical Lectures and Essays.* By Sir James Paget, Bart., F.R.S., D.C.L. Oxon., LL.D. Cantab. Edited by Howard Marsh, F.R.C.S.

which would be out of place in a formal treatise; and his observations have the additional interest which is always given by an immediate application. In this way also the listener's intellect is stimulated to the filling up of suggestive outlines, or the pursuit of indicated paths of inquiry. This it is which gives so great a value to the conversation of men of learning and wide observation, some of whose best thoughts have been thus transmitted. Unfortunately the pace of our present mode of life gives but little opportunity for sustained rational conversation. People in the present day have not time for the "talk" whereby men such as Johnson or Coleridge used to recreate and improve themselves and others. The result of this has been an increase of periodical literature, containing short essays and papers, corresponding in style very much to the conversation of former days. It is well for us, then, when one having such exceptional materials for and such unexampled powers of teaching, as distinguish Sir James Paget, gives to us in a collected form, some of his occasional and less systematic lessons, and thus enlarges, as it were, the circle of his pupils. The volume of 'Clinical Lectures and Essays' which Sir James Paget has recently published is modestly prefaced by the statement, that it is not intended for "those who are in large surgical practice or familiar with surgical literature. Its chief purpose will be attained if it be useful to students and to those who have too few opportunities of studying surgery in either large practice or large books." But we think there are few in the profession who will not be glad to profit by the sagacious reflections and mature advice contained in the book, and to possess, in a convenient form, the valuable essays which are therein gathered from many scattered sources.

These essays also have a high value as models of method and style. A man of high scientific attainments recently expressed to the present writer his opinion that the members of the medical profession excelled chiefly in their powers of immediate deduction, but were, as a rule, not skilful in carrying on sustained chains of reasoning. This we believe is, generally speaking, true; but Sir James Paget has given many evidences of the possession of very high reasoning powers, and the student may gather from his writings numerous examples of cautious and well-sustained argument, as well as of subtle analysis and accurate deduction. Moreover, it may be noticed with what modest reticence he abstains from attempting explanations when the facts are insufficient—an example too little followed by many medical writers; and the style has, besides the clearness which belongs to a thorough comprehension of the subject-matter, a classic eloquence which makes it most attractive reading.

Mr. Marsh, besides having edited the volume, has added some valuable notes, which we shall notice in their place.

The first essay is on the various risks of operations, and gives us many useful notes whereby we may distinguish those patients which are, from those which are not, favorable subjects for operations. Those who recover best from operations are not they who are most healthy for active life, but boys between the ages of twelve and sixteen years, and chronic invalids who have lived carefully and temperately, and "to whom an operation brings no great change of habits." The boys, we presume, owe their good recovery very much to the soundness of their bodies, which have not yet been exposed to much damage by wear or dissipation; the chronic invalids to their careful living and the little change of habits involved by an operation.

Children are likewise, in many respects, free from dangers which beset their seniors, and are in danger chiefly from the shock of an operation. They bear pain ill, and, as Mr. Marsh points out in a note,[1] may be reduced, by a few hours of acute pain, to a state of dangerous collapse. Mr. Marsh very properly, therefore, advises the free use of anæsthetics and of opium—advice which we would earnestly endorse.

Children take anæsthetics very well, and we possess in ether one which not only has the advantage of producing insensibility to pain, but which also, by its stimulant effects, acts as an antagonist to shock. We think that the value of opium in the traumatic fever and nervous irritability, following operations in children, is by no means sufficiently appreciated. The effect upon them of small and frequently repeated doses is most excellent, and we quite agree with Mr. Marsh, that "it is, perhaps, the most valuable medicine that children ever take." Mr. Marsh also questions the accuracy of the opinion that children bear the loss of blood badly. We consider that their large reparative power enables them rapidly to make up the loss; but we think also that they are in graver danger at the time of the hæmorrhage, than adults would be from a proportionate bleeding, on account of the profounder impression made upon the nervous system. It is quite true, as Mr. Marsh says, that convulsions do not often follow large hæmorrhages in children; but coma does, as we have several times seen, after not very copious bleeding.

The author remarks that children are much less liable than adults to pyæmia after operation, and the editor adds that they more often recover when it does occur. This should encourage us to operate on children in the early stage of pyæmia, when

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[1] P. 399.

by so doing we can remove the source of the blood-poisoning. We have ourselves seen a child, who was unquestionably suffering from pyæmia, make a rapid recovery from the time when a diseased limb was amputated. The tuberculosis which sometimes seems to be set up by an operation may, we think, be occasionally due to the interference with old caseous material which the operation often involves.

Age is, of course, a source of danger to the subjects of operations, and the author wisely reminds us that we must judge of age, not by years, but by the amount of degeneration present. Old persons bear the loss of blood badly, and we are warned against keeping them long recumbent, or over-feeding them. "They that are fat and bloated, pale, with soft textures, flabby, torpid, wheezy, incapable of exercise, looking older than their years," are characterised as "very bad" subjects for operations. The scrofulous are said to exhibit, in reference to operations, chiefly want of healing power. This is no doubt true, and the rapid recovery which follows the removal of the majority of joints affected by chronic disease, is evidence that many of such cases are miscalled "scrofulous." The syphilitic and the gouty do not seem to incur any special risk from operations, excepting in so far as the latter are liable to have granular kidneys or other degenerations; neither do the cancerous nor the plethoric. The very fat, especially those who overeat and drink, and have pendulous bellies and defective portal circulation, are referred to as a very bad class for operations. Here the author gives a wise warning concerning the prevalence of habitual soaking, and the fatal harm that many persons, who pass for highly respectable people, thereby do themselves.

Dyspepsia is spoken of as increasing the risk of chloroform or ether sickness. This may, perhaps, be so; but we think that renal disease is by far the most common cause of the after-sickness from anæsthetics. We not only agree with the author that constipation for a few days after an operation is harmless, but would go further, and say that it is very often beneficial to prevent the disturbance accompanying an action of the bowels, and that after an operation for strangulated hernia it is desirable to give opium for this purpose.

We think that Sir James Paget rather over-estimates the danger of operating on those with amyloid enlargement of the liver, which, when coincident with long-continued suppuration, may be even an additional reason for removing by operation, the disease which may be its cause.

We are surprised, moreover, to see that he is rather in favour of operating for fistula in phthisical patients; and we question, indeed, whether many persons in ill health, even when not

phthisical, are not more harmed than benefited by the "free incisions" which are usually thought applicable to sinuses and abscess about the anus.

The danger of operating on the subjects of albuminuria is alluded to, and there is a useful warning against incautious catheterism, which, with some remarks on operating upon the insane, the pregnant, and the suckling, bring this subject to a close. Then follows a chapter on "The Calamities of Surgery," which, it is shown, are sometimes to be averted by careful attention to details, but are sometimes by no skill to be avoided. The author justly says that a man who is to be submitted to an operation ought at least to be inspected with a care equal to that expended upon a candidate for life insurance, and he recommends for guidance the questions which an insurance office requires to be answered.

There is a great element of safety in the consultations, usually held in hospital practice, upon all cases in which an operation is proposed.

We regret to notice that Sir James Paget still speaks somewhat doubtfully of the advantages of ether over chloroform, although, in a note, he states that "for the last two years I have used only sulphuric ether, or, for short operations, nitrous oxide gas or ether-spray." Certainly, deaths from anæsthetics are among those calamities of surgery which may be notably reduced by the careful selection of the safest available agent. Nothing can be better than nitrous oxide for short operations, and it might be much more frequently used for such than it is at present; but for operations of longer duration, it has been abundantly shown that ether is by far the safest anæsthetic which we at present possess, and that it can be given as easily and conveniently as chloroform. Moreover, it is less liable than chloroform to produce after-sickness, and is, by its stimulant effect, antagonistic to the shock of an operation. Examples are given of the value of attention to small things in surgery, and of the lessons that may be learned from failures and mistakes. "But there are some people who seem to have a happy art of forgetting all their failures, and remembering nothing but their successes, and, as I have watched such men in professional life, years have always made them worse instead of better surgeons. They seem to have a faculty of reckoning all failures as little, and all successes as big; they make their brains like sieves, and they run all the little things through, and retain all the big ones which they suppose to be their successes; and a very mischievous heap of rubbish it is that they retain."

The short essay on "Stammering with other Organs than those of Speech" gives the key to understanding a class of cases,

that without it, would often be puzzling. This is the note of these cases:—"Stammering, in whatever organs, appears due to a want of concord between certain muscles that must contract for the expulsion of something, and others that must at the same time relax to permit the thing to be expelled." Instances of this discord or stammering are given, affecting the urinary organs, the œsophagus, and the rectum.

The next lecture is on "Cases that Bone-setters Cure." Some of the most successful of such cures are due to the rupture of adhesions between the joint-surfaces, others to the more gradual lengthening of shortened structures outside the joints. The author gives some useful guides towards the discernment of the cases that are suited to such treatment. For it must be remembered that although the public hear but little of the failures and injuries done by the "bone-setters," yet such treatment is sometimes followed by the gravest mischief. We doubt whether adhesion or ulceration takes place in joints as the result of mere immobility, though the surrounding soft parts may become contracted or wasted; and we think, therefore, a careful inquiry should be made in all cases of stiff joints for the occurrence of even the slightest inflammatory attack, and that this should very much influence us as to whether the joint should be suddenly, or gradually moved. The gradual stretching of an adhesion, as a rule, does harm, whereas its rupture is often at once curative; but when the stiffness of a joint depends upon the structures around, and not within it, the slower method of rubbing and exercise is much more successful than any sudden and extended movement, which, indeed, may do much harm. Sir James Paget refers to a case, related by Mr. Butlin, of ankylosis of a knee-joint which was long kept straight for the treatment of a fractured femur; but the case is not a conclusive one, for there had been a recent fracture of the lower end of the femur, when the knee-joint might possibly have been injured; these were ulcers of the articular cartilage which were surrounded by vascular patches, and the bone was somewhat softened. Moreover, Dr. Reyber has shown that absorption of the cartilage of a fixed joint takes place when the parts are not in contact, and not where they are in apposition, so that the wasting of the cartilage is due rather to loss of function than to pressure.

The lectures on "Strangulated Hernia" are full of valuable experience gathered from the author's notes of his cases, but contain less of novelty than the rest of the book. Among the signs of strangulation commanding the operation for hernia, the most reliable is said to be vomiting, the importance of which, we believe, cannot be too much insisted upon.

With regard to the peritonitis occurring in connection with strangulated hernia, Mr. Marsh, in a note, alludes to Mr. Hutchinson's opinion that peritonitis very rarely occurs before the operation, unless there be perforation, and that when it occurs after the operation it is chiefly caused by the return of inflamed intestine and flakes of lymph, and by the injury inflicted by fingers, &c., introduced into the sac. We admit the rarity of peritonitis before the operation, unless there be perforation; and we believe that by far the most common cause of its occurrence after the operation, is the return of inflammatory products into the peritoneal cavity.

Recent experiments have shown the intense and rapid inflammation that may thus be set up, and we think this is one of the strongest arguments for opening the sac in the majority of operations for strangulated hernia. If the hernia has been long strangulated, inflammatory products are almost certainly found in the sac, and, moreover, the condition of the intestine must be the more doubtful; and if an operation is called for after a very short time of strangulation, the case is probably an acute one, and inflammatory exudation is again probable. To this lecture Mr. Marsh appends an excellent note on the "aspiration" of strangulated hernia, and gives, we think, a very just estimate of the value of that method of treatment. The paper on "Chronic Pyæmia" describes several cases of that disease, one of which extended over three years. Its frequent association with acute necrosis is pointed out. "The frequency of death from acute pyæmia in this disease is well known, yet I think there are no cases of pyæmia in which death is more often escaped."

Perhaps the most interesting and valuable lectures in the volume, are those on "Nervous Mimicry;" they notably display the sagacity of the author. Herein we are warned against the common error that neuromimesis "is to be found only or chiefly in the silly, selfish girls among whom it is commonly supposed that hysteria is rife or an almost natural state." Rather are we to look for it in those of unusual mental or emotional character, and to remember that "I cannot," though it often looks like "I will not," really may be "I cannot will." Very important in aid of diagnosis is the family history, which often gives evidence of ancestral or consanguineous insanity, and the author justly lays great stress upon the hyperneurotic diathesis of these patients, and the fallacy of supposing "that nervous mimicry or hysteria, or any of the allied forms of disease, can be referred to any malady of any other part than the nervous system." But it must also be remembered that there are numerous cases of trivial disease or injury, made to seem severe by hysteria or other nervous fault." Excellent advice is given for

the diagnosis of the real from the mimic disease, and there is a description of a nervous dilatation of arteries simulating aneurism, of which we have seen several examples, but of which we have not elsewhere met with a description.

The lecture on "Carbuncle" asserts the uselessness of the cutting, stimulating, and feeding which used to be thought necessary; and the value of fresh air and careful dressing of the wound. Our experience is quite confirmatory of this, and we think that, thanks to Sir James Paget's teaching, the more rational treatment he advises is gaining ground. The mortality of the disease has been much over-estimated, and in Sir J. Paget's opinion "there is no other disease of the same extent and general severity which is attended with so little risk to life."

This statement refers, of course, to uncomplicated carbuncle, for, like any other extensive suppuration, it may be the fatal addition to existing disease, as, for instance, it not unfrequently is to diabetes. The fatal affection called "facial carbuncle" was, when the lecture was first given, considered by the author as a separate disease, but he now "feels nearly sure that the disease of the lip is true carbuncle which, because of some peculiarity in the textures of the lip, especially in young persons, is peculiarly apt to infect the blood and generate acute pyæmia." To the remarks on dressing carbuncles, we would add that nothing has seemed to us so efficient, both for comfort and progress, as the application of the oakum now used for dressing wounds.

There then follows a most admirable chapter on what the author has well named "sexual hypochondriasis." It seems to us a model of the method in which such subjects should be treated, namely, by straightforward, conscientious, plain-speaking. It will be very helpful to many of less authority, to be able to quote Sir James Paget's denunciation of the false teaching of Lallemand and his followers; and, we doubt not, that many wrong and harmful opinions will be neutralised by the excellently wise words in which Sir James treats of some of the difficult questions connected with this subject.

A paper on "Gouty Phlebitis" is founded upon several interesting cases which are related. Among other results of this affection, one not commonly known, is the hypertrophy of the muscles of the limb to which the author calls attention.

The essay on "Residual Abscesses" describes a class of cases frequently seen, but often misinterpreted, and gives, we believe for the first time, the real explanation of their nature. "Under the name of residual abscesses" are included "all abscesses formed in or about the residues of former inflammations. Most of them are formed where pus, produced long previously, has

been wholly or in part retained and become dry, or in some form obsolete; but some of them, it is probable, are found in the thickenings, adhesions, or other slowly organised products of inflammation long past." These abscesses remind us of what we think is too often forgotten—that the effects of disease frequently remain when the disease itself has been long at an end. We think that this is the explanation of some of the cases spoken of by Sir J. Paget in a subsequent chapter as "senile scrofula," in which, because of advancing years or broken health, some of the latent products of old inflammation are the seat of fresh suppuration. We cannot but think that John Hunter was right in regarding scrofula as a disease of youth.

The paper on "Dissection Poisons" is a description with commentary, of the illness from which its author suffered, in consequence of poisoning during a post-mortem examination of a patient dead of pyæmia. Sir James points out the curious but indubitable fact that immunity from this kind of poison is acquired from constant contact with it, and that such immunity may be both local and general, also that after a time this immunity is lost. Most of those much engaged in morbid anatomy can confirm this. In speaking of his own liability to pneumonia the author usefully reminds us "that there is no disease so specific but that its signs may be confused or complicated with the things that are peculiar to the patient." Thus are explained the various eruptions and other troubles sometimes educed by, and bringing discredit upon, vaccination; "they come out from the personal constitutions of the several patients, which are disturbed by the vaccination, as they might have been by anything else producing some slight fever.

A short paper on "Quiet Necrosis" (we cannot help a passing expression of our admiration for the happy titles which Sir J. Paget gives to his essays), describes the exfoliation of bone and cartilage which sometimes occurs "without any of the attendant phenomena of either inflammation or fever."

The essay on "Scarlet Fever after Operations" draws attention to the fact that "there is something in the consequences of surgical operations which makes the patients peculiarly susceptible of the influence of the scarlet fever poison." The disease in these cases is variously modified, but in all of them the period of incubation is unusually short. In a note on this subject Mr. Marsh gives some striking facts collected from the records of the Hospital for Sick Children. We think he clearly shows that the disease is really scarlet fever, and that "both the explanations of Sir James Paget are correct, though of course in different cases." These explanations are as follows: "Either the condition induced in a patient by a surgical operation is one

that gives a peculiar liability to the reception of an epidemic or contagious poison, and any one of these, being imbibed immediately after the operation, produces its specific effect in much less than the usual period of incubation; or else those who suffer with scarlatina within a few days after operation, had previously imbibed the poison, but would not have manifested its effects so soon, if at all, unless their health had been exhausted or disturbed." These facts have a great practical importance for those engaged in operative surgery, and should be especially remembered in connection with plastic operations, wherein immediate union is essential to success. We have certainly seen a considerable number of such operations fail from the occurrence of scarlet fever immediately after the operation. The remainder of the volume consists of "Notes for the Study of some Constitutional Diseases." These notes are extremely suggestive, and will furnish the thoughtful reader with rich material for reflexion and investigation. They do not profess to be more than "fragments," but they display in a notable degree that sagacity and exact observation—that *ἀγχίνοια* and *ἀκρίβεια*—which give so great a value to all the writings of their author, and which make these "fragments" of more worth than many a bulky tome.

We resist the temptation to quote any part of these notes, but would point out as especially interesting and suggestive, the remarks on "the variation of constitutional diseases in hereditary transmission" and those on "the evolution of diseases."

We recommend our readers not to lose the pleasure and instruction to be derived from a perusal of this book, which we consider one of the most valuable recent additions to surgical literature. Mr. Howard Marsh, besides editing the work, has increased its value by the admirable notes to which we have alluded, and by a copious index; he also gives some useful references to the literature of some of the subjects treated.

IV.—Medical Missions.

Some of our readers may ask, What is a medical mission? It is well, therefore, that we should at the outset explain the

[1] 1. *The Quarterly Paper of the Edinburgh Medical Missionary Society.*

2. *The Medical Missionary Journal.* Published monthly by Dr. Burns Thomson.

3. *Memorials of James Henderson, M.D., F.R.C.S. Ed., Medical Missionary to China*, 1872.

4. *Seedtime in Kashmir; a Memoir of W. J. Elmslie, M.D., F.R.C.S. Ed., Medical Missionary in Kashmir.*

meaning and scope of the term. A medical mission is an agency for ministering to the sick; and, while their minds are softened and touched by kindness, or by the physical relief they have experienced, taking the opportunity to inculcate the first principles of Christianity. A medical mission has thus a twofold aspect. It addresses itself to the cure of the body and at the same time to the enlightenment of the mind. But this is not the place to dwell upon the religious aspect of the subject. However scriptural and reasonable such a mode of extending the limits of Christendom may be, a medical review is not the fittest place for enlarging upon it. We shall, therefore, confine ourselves to speaking of medical missions as they aim at the relief of sickness and disease. This will afford us a sufficiently wide scope and a sufficiently varied field of observation. It will carry us back from our own days to the commencement of the Christian era; it will transport us from the large cities of our own country far off to China and Japan; and from the aggregated masses of our own crowded courts to the secluded Zenanas of Hindostan.

In dealing with the subject we shall give, first, a slight sketch of the history and progress of medical missions, and then we shall speak more in detail of their rapid development during the last thirty years. For it is their recent development, and the variety of forms in which they are now advancing, that gives to medical missions their present interest, and makes them a suitable subject for consideration by such a review as this.

We have said that our subject carries us back to the earliest days of Christianity. We need but remind our readers how the Divine Founder of our religion "spake of the kingdom of God, and healed them that had need of healing;" and, further, that He enjoined upon His disciples "to preach the kingdom of God and to heal the sick."

In the third and fourth centuries many instances are recorded of Christians who devoted themselves with the utmost assiduity to the relief of sufferers from the plague and other epidemic diseases; amongst these may be mentioned Columba, the noted apostle of the Picts and Scots, whose skill and success was so remarkable as to make many regard his cures as miracles—a thing which has often occurred to medical missionaries from his time to the present.

Later on in the middle ages many societies of monks were instituted to provide physical and spiritual relief for the sick and the ignorant. Thus one order was founded in France to furnish nurses for persons attacked by St. Anthony's fire, as erysipelas was then called. This disease was in those days a far greater scourge than it now is, and caused a terrible mortality

throughout Europe. Others, both male and female, devoted themselves to the care of lepers, and founded large establishments in which to receive and nurse their patients. Jacob of Vitry says of such, "For Christ's sake they bring themselves to endure, amidst filth and disgusting scents—by driving themselves up to it—such intolerable hardships that it would seem as if no sort of penitential exercise which man imposes on himself deserved a moment to be compared with this holy martyrdom." The Jesuits also, following the example of Loyola, have always paid especial attention to the care of the sick in their foreign missions; and to this, no doubt, a part of their success may be traced.

It must be borne in mind that in the middle ages learning, in all its branches, was in the hands of the ecclesiastics. Hence it was almost a necessity that all measures for the relief of the sick should emanate from them; and that every one who was attracted by the study of medicine, or who had a natural aptitude for nursing, should join himself to one of the religious orders, and exercise his vocation under the protection of the church. But though some of these orders doubtless owed their origin to a genuine religious zeal in their founders, yet in course of time they so far degenerated that even Papal edicts were issued against the fraudulent tricks of the monks who went about collecting for the "spittals" large sums of money, of which but a small portion was ever expended upon them.

The history of British medical missions may be traced back to the year 1701. The earliest effort in this direction was due to General Codrington, an English officer, who was the possessor of estates in the West Indies. A portion of these he left to the Society for the Propagation of the Gospel in Foreign Parts in the following terms:—

"General Codrington gives and bequeathes his two plantations in the island of Barbadoes, and part of his island of Barbuda, to the Society for the Propagation of the Christian religion in foreign parts, created and established by his good master King William the Third; and desires that the plantations should continue entire, and 300 negroes, at least, be always kept thereon, and a convenient number of professors and scholars maintained there, who are to be obliged to study and practise physic and chirurgery as well as divinity, that by the apparent usefulness of the former to all mankind they may both endear themselves to the people, and have the better opportunity of doing good to men's souls while they are taking care of their bodies; but the particulars of the constitution he leaves to the Society composed of wise and good men."

Many difficulties attended the carrying out of this will, but finally an establishment was formed and supported out of these

estates consisting of a President and twelve scholars, and allowances were made to those who wished to prosecute their studies in England. The annual value of the property so bequeathed is said to have amounted to £2000, a larger sum of money than has ever been devoted to medical missions by any single individual before or since.

A little later in the eighteenth century two medical men, Dr. Hocker and Mr. Rueffer, were sent by the Moravians to Persia. They might have settled at Ispahan with every prospect of advantage to themselves, as the Persians set a high value upon the skill of European physicians, but, as they found no scope for missionary exertions, they fell back upon Cairo. Mr. Rueffer died before reaching that city in consequence of the hardships which they underwent, while Hocker continued the practice of his profession in Egypt for thirty years.

In 1787 Mr. John Thomas, surgeon to the "Oxford," East Indiaman, who had made several voyages to Calcutta and become deeply interested in the miserable condition of the Hindoos, resolved to remain in Bengal, and labour among the natives. He was a most devoted missionary, and his surgical skill gave him great influence. Multitudes flocked to him for advice, many from distant parts, and when he travelled through the country his progress was often hindered by the crowds who came to him to be treated; and in consequence of the interest in Christianity which his words, no less than his benevolent work, had created, the well-known Dr. Carey was sent out by the Baptist Missionary Society in 1793.

A few years later Dr. Vanderkemp was sent to South Africa by the LondonMissionary Society. He laboured among the Caffirs with the utmost zeal, and his medical skill won for him a high place in their affections. The strong footing which Christanity has since obtained in South Africa is probably in no small measure due to the influence of this medical missionary.

The first effort in this direction made in China was in 1819, when Dr. Livingstone, a surgeon of the East India Company, called the attention of the Rev. Dr. Morrison, an agent of the London Missionary Society, to the Chinese system of medicine. He was struck by the number of medicinal plants which were exposed for sale in the markets, and which were not known to the European pharmacopœia. Dr. Morrison took up the subject warmly, and appears to have seen how much it might assist him in gaining the confidence of the Chinese if he took some steps to relieve their bodily sufferings. He, therefore, purchased a complete Chinese medical library with an adequate assortment of Chinese medicines; he engaged the services of a Chinese physician and apothecary, as well as a herbalist, whose entire

stock he purchased for Dr. Livingstone's examination. He then opened a dispensary at which he himself endeavoured to be present two or three hours daily, and Dr. Livingstone also frequently gave the patients the benefit of his services. This effort appears to have originated chiefly in the desire to ascertain what value there might be in the Chinese medical system, and how far it might be advantageously studied by a European surgeon. It was at least a matter of curiosity to see whether their medical system was worthy of the name, and whether it was possible that their *materia medica* might contain some articles of real value. The conclusion arrived at was that 2000 years ago the Chinese were greater proficients in medicine than any of the nations of the West; but that since that time few improvements have been introduced, whilst an abundant crop of errors has sprung up in every department of practice.

In 1827 Dr. Colledge opened an eye infirmary at Macao. Eight years later a similar institution was commenced at Canton by Dr. Peter Parker, an agent of the American Board of Missions. The success of his treatment produced a great sensation, and patients of all ranks flocked to the hospital from all parts of the empire. In the course of two years 4575 persons passed under his care. Reports of the cases were published quarterly in the 'Chinese Repository,' which contain many interesting particulars relative to the diseases of the eye, which are so common in the "celestial empire." He did not, however, confine his attention altogether to ophthalmic practice. Indeed his success in all branches of surgery was so great that we cannot be surprised at the esteem in which he was held by the Chinese. The following extracts from his reports will show both the importance of the operations which he sometimes had to undertake and the mode in which the natives expressed their gratitude. "They were much struck," he writes, "by the case of a beggar from Macao, who had a tumour of extraordinary magnitude upon the side of his face and head. When it became so large as to disable him from labour, he had no resource but that of begging. This burden, though one that would weary a man to bear an hour, he could not put off for a moment day or night. He had long been a loathsome and pitiable object to the citizens, and when I was at Macao, as I passed him in the streets, he presented written appeals, from unknown authors, to my sympathy, and requests that he might be relieved of his burden. The tumour, measuring 2 feet 6 inches in circumference, and weighing a few ounces short of nine pounds, was lately removed. The man quite recovered in three weeks. He is now porter to the hospital, where he acquits himself well. The

magnitude of the operation, the elliptical incisions being about eighteen inches in length, and the adhesion of the base over the carotid artery and the parotid gland being deep and strong, rendered it impossible it should be performed without solicitude."

The following is a specimen of one of the scrolls of gratitude which were not unfrequently presented to the doctor by patients who had been cured by him. Dr. Parker writes:—

"February 26, 1849, Chusher, a manchu, æt. 54, had a fungoid tumour of the size of an orange situated upon the back and spine. The tumour was readily and successfully removed. Before leaving the hospital the patient made repeated solicitations to be allowed to send an artist to take the portrait of the surgeon. His importunity was at length acceded to, and soon there was presented a portrait, taken in water colours, by the side of which, on the same canvas, was the following inscription in poetry, and an account of his case, and what he had seen in the hospital:—

"What man is that? America's noble and disinterested man, who does to others as he would that others should do to him. His country is different from ours, his feelings are the same. In all distresses and diseases he feels the sorrows and joys of others as though they were his own. Those cases which require the use of instruments, and which are difficult to others, are easy to him. He cherishes a mind that is divine, and bears the visage of Budha; a full halo of glory surrounds his deeds, and he deserves immeasurable longevity. Parker's meritorious virtues are as numerous as the sands of the ever-flowing river. I denominate him as Ye-Sir. What say you, yes or no?"

Indeed, China has always been a favorite field for the work of the medical missionary. The people are so opinionated, and so proud of their semi-civilisation, that it is difficult to get them to give a fair hearing to the religion of Christendom, but they can at once perceive the value of the science which the medical missionary brings along with him. In 1838 a local medical missionary society was formed—"The Medical Missionary Society in China"—of which Dr. Colledge was (and still is) the president. The thirty-sixth Annual Report, which has just reached us, is a most interesting pamphlet. With its details of work done at the various dispensaries and hospitals in connection with the Society, its Chinese woodcuts of remarkable cases, with their explanations in Chinese characters, and its allusions to the peculiarities of the natives, it gives in a brief space a vivid picture of what medical missions are doing in a far distant field.

In 1839 the London Missionary Society sent out Lockhart and Hobson. The former, who has now returned to this coun-

try, laboured at Shanghai for nearly twenty years. Mr. Hobson settled at Canton, and opened a hospital there in a good situation, easy of access both by road and water. There was a good dispensary with separate entrances for men and women, and there were twelve rooms on the ground floor, capable of containing 45 beds, besides extra accommodation for a hundred more patients, if at any time it should be needed. This hospital was resorted to by about 20,000 patients annually. Mr. Hobson paid particular attention to the education of Chinese youths in the principles of European medicine, and for this purpose published several small works in Chinese on anatomy, surgery, medicine, midwifery, comparative physiology, &c.

He also trained several young Chinese in the science and practice of medicine, and they afterwards became his assistants.

In 1840 Dr. Parker visited this country. Articles which had appeared in newspapers on the subject of the medical work going on in China had attracted the notice of several members of our profession, and had prepared them to take an interest in the matter. Dr. Parker had interviews with most of the leading medical men of the metropolis, and one result was that the Royal College of Surgeons undertook to educate, free of expense, such Chinese youths as might be sent to this country for medical training, and three scholarships were founded at King's College for the education of medical missionaries.

The most notable result, however, of Dr. Parker's visit to this country was the formation of the Edinburgh Medical Missionary Society, which was founded in 1841, partly to assist the Chinese missions, and partly to aid the Syrian Medical Association. This brings us to what may be considered the modern development of medical missions, for to the Edinburgh Society may be traced almost all the work which has been done in this direction by this country during the last thirty years. The first President was the late Dr. Abercrombie, who took a lively interest in its proceedings and frequently attended its meetings. But Dr. Abercrombie was not the only distinguished medical man who actively supported the infant society. Among those who have been its friends, and who have passed away, we may mention Dr. Alison, Dr. Coldstream, Professor George Wilson, Professor James Miller, and Sir James Simpson. While a glance at the present list of directors shows that many of the most eminent men in Edinburgh take an active part in its promotion, it has at various times arranged for the delivery of lectures and addresses to students on medical missions, and it publishes a *Quarterly Paper* giving current information upon the subject. To these sources we are indebted for many of the details contained in this article. The Society maintains a mis-

sionary dispensary in the Cowgate, and also a training institution in George Square. There are now in this institution fifteen students, who are prosecuting their studies at the University or at the College of Surgeons, with the view of becoming medical missionaries. The Society also maintains at the present time three medical missionaries abroad—Dr. Elder, at Madras, Dr. Palm, in Japan, and Dr. Vartan, at Nazareth. Indeed, the operations of the Society in all its branches have been so steadily increasing that the need of larger and more convenient premises has been much felt, and an effort is now being made to raise £10,000 in order to erect a suitable building to embrace both the dispensary and the training institution, and this new building it is proposed to associate with the name of David Livingstone, the great African explorer and medical missionary.

To those who have followed the course of medical missions during their recent development there is no name more familiar than that of Dr. Burns Thomson. For many years he was the superintendent of the work of the Society in Edinburgh. He had been one of their earliest students, he wrote a prize essay on the subject, and his devotion to the cause and his energy of character led to his appointment to this important post. Under his guidance the Society made rapid advances both at home and abroad. Though he is no longer connected with it he still devotes himself to medical missionary work, and carries on a dispensary of his own and a convalescent home. He moreover supports three agents abroad—two medical men and one trained nurse in Madagascar. He publishes monthly the 'Medical Missionary Journal,' which was the first serial devoted to this subject, and much of the information contained in this article has been gleaned from its pages.

So early as the year 1824 the attention of the London Society for Promoting Christianity among the Jews was directed to the importance of establishing a medical mission. In that year they sent Dr. Dalton to Jerusalem with this object; but in 1826 he died, and the plan appears to have remained in abeyance for some years. In 1842 Dr. Macgowan was sent out as the Society's medical agent, and in 1844 a hospital containing 25 beds was opened. From that time to the present it has been actively carried on. It is now under the medical charge of Dr. Chaplin, and last year he and his qualified assistant saw 7771 out-patients, besides making 11,445 visits to sick Jews at their own houses. During certain seasons Dr. Chaplin makes a tour of Palestine, halting for a short time at each of the towns where the Jews chiefly congregate.

It may be well here to explain the way in which the work of the missionary dispensary is carried on in Edinburgh, as it is the

model upon which other institutions of the same kind have been framed, both at home and abroad. The accommodation provided is much the same as that of ordinary dispensaries, and consists of a waiting-room, a consulting-room and a pharmacy. The patients assemble as usual in the waiting-room, and at a fixed hour a passage of Scripture is read and explained in a simple manner. A short prayer concludes the brief service; and then the patients are seen separately in the consulting-room, and prescribed for. Besides receiving patients at the dispensary, the medical man and his assistants visit the sick at their own houses, which indeed constitutes a most important part of the work of all ordinary dispensaries. These various opportunities of seeing the patients give the medical missionary the means of becoming intimately acquainted with their moral condition, and he is able to graft upon the dispensary different classes, meetings, &c., for elevating and Christianising the population among whom he labours. In those foreign stations where the dispensary has grown into a hospital the work is carried on in substantially the same manner, though it will easily be believed that the hospital is found a more efficient basis; and in many cases the ministerial duties are conducted by a clerical missionary, who acts somewhat in the capacity of chaplain to the institution. Dr. Burns Thomson has very properly laid great stress upon the importance of training missionary nurses, and this is now being done not only in Edinburgh but also at several of the foreign stations.

To many persons it has appeared desirable that the general treatment of the sick poor should be carried on by means of medical missions, and that direct moral and religious influences should accompany the practice of medicine and surgery among the ignorant and degraded classes of society. This has led to the establishment of medical missionary dispensaries and hospitals in many of the large cities of the kingdom. Starting from Edinburgh as a centre, they are now to be found in Aberdeen, Glasgow, Liverpool, Manchester, Bristol and London; while if we cross the channel we hear of them in several of the populous towns of Europe, and, if we travel yet further eastward, we meet with them in Syria, in Madagascar, in India, in China, and in various other places.

We have named at the head of this article the two serials which are devoted to the spread of information upon the subject of medical missions, and also the memoirs of two medical missionaries who have lately passed away while they were still actively engaged in their self-denying labours. It is not possible for us to do more than to touch upon a few of the salient points of our subject. But if any one wishes to obtain a comprehensive

view of what medical missions are doing he should study these periodicals, and if he desires to see how they are carried out in detail he should read these biographies. The men whom they pourtray were both of them remarkable, and their lives were full of interest, apart altogether from their bearing upon our present subject.

Dr. Henderson was the son of a poor labouring man in the Highlands of Scotland. His mother was left a widow when he was scarcely three years old, and his early years were spent in much poverty. His mother taught him to read, but this was all the instruction he had. As a young boy he was employed by a farmer to herd cattle. A little later he was engaged by a country doctor as groom. At this time he began to have an earnest longing for more information, and subsequently, when he had obtained the situation of footman in a gentleman's household, he set to work in the most diligent and self-denying manner to educate himself. The butler, who had himself been trained for a higher position, was a kind friend to him, and rendered him great assistance. It was at this time that he first entertained the idea of aspiring to a University training to fit himself for the ministry. He had, however, no friends to aid him, and he was discouraged from the attempt by all whom he consulted. But he adopted as his principle, "There is nothing that has ever been accomplished by man in past times which I, as an individual, may not accomplish, provided that other things are equal;" and having by the most rigid economy laid by a small sum of money, he gave up his place, and took lodgings in which he shut himself up all day, and worked and studied incessantly from early morning till past midnight. He lived in the most frugal manner, spending for his board and lodging no more than 4s. 6d. a week. After five months he decided on going to Edinburgh, and there he was fortunate in obtaining a situation as footman to a lady who paid him good wages, while she left him a great deal of time at his own disposal. This leisure he employed in study, and he engaged the assistance of first-rate masters in Latin, Greek, and mathematics. In this way he prepared himself for college, and saved sufficient money to pay his fees. He was now 25 years of age. He had received no encouragement to enter the ministry, while his attention had often been drawn, amid his humbler friends and acquaintances, to the beneficent aspect of our profession. He felt that it afforded opportunities for doing good quite unknown in any other calling; and it became the object of his ambition to qualify himself for it. In 1855 he entered himself at Surgeon's Hall, gave up domestic service, and studied with his wonted assiduity throughout the whole of his course, and acquitted himself most

creditably. When he received his diploma he was offered a practice that would have yielded him £700 a year. But he had already made up his mind to be a medical missionary, and, after a very short delay, he was engaged by the London Missionary Society to proceed to China. When he presented himself before the Board in London his self-possessed manner, gentlemanly bearing, and frank answers, excited special interest in him, while his manifest intelligence and proved energy of character secured his immediate nomination. His destination was Shanghai, where he succeeded to the work of Dr. Hobson, and his time was soon fully occupied by the numerous patients who flocked around him. During the first year he prescribed for more than 16,000 persons, whilst 169 patients were treated in the wards of the hospital. He was assisted by Chin Foo, a native apothecary and house surgeon, of whom he wrote :—

"He has been in the hospital now about eight years, and assisted Drs. Lockhart and Hobson. He is attentive to all his duties, very intelligent, and kind to the patients, has carefully read all Dr. Hobson's medical works in Chinese, and, were it not that he wants practical anatomy, he would be a good surgeon ; but, owing to the stupid prejudices of the Chinese, he has never even seen the interior of a dead body. I have tried to teach him from anatomical plates, but these are not sufficient. He can, however, perform the minor operations well, under my direction."

After about two years' residence in Shanghai Dr. Henderson returned to this country to be married. But he remained here a very short time, and was back again at his post in eight months. From this time till the date of his death in 1865 he continued his work at the medical missionary hospital. His popularity among the Chinese was great, and the number of his patients steadily increased. In 1864 he was made F.R.C.S. Ed., on account of his scientific acquirements and of his researches as vice-president of the Chinese Branch of the Royal Asiatic Society.

Dr. Elmslie's career was in some respects not unlike that of Dr. Henderson.

He was born in Aberdeen in 1832. His father was a shoemaker, and, as soon as his son was old enough to help in the work, he insisted on his beginning to learn his trade. He would not hear of his being sent to school. If it had not been for the encouragement of his mother, and his own energy, he would have remained ignorant even of the rudiments of learning. She did all she could to help him, and in the evenings after work his young friends often taught him something that they had just learnt at school. He himself was eager for instruction, and used frequently to fix a book in front of him and study

while he was at work. He became very skilful in his trade, and thus earned money which enabled him at the age of sixteen to enrol himself as a pupil in the grammar school of Aberdeen. But still during his spare hours he was obliged to work as a boot-closer. Notwithstanding these disadvantages, in his second year he gained a bursary by competition, and at the end of the next session he carried off the first Greek prize. In 1853 Elmslie passed from school to college. His father's failing health now made him more than ever dependent upon his son's exertions. He, therefore, undertook an engagement to teach in a school in Aberdeen, and he had also several private pupils. Being a first-rate student, and of gentlemanly manners, he had no difficulty in getting as much employment as he wished. After he had taken his degree in arts he gained another bursary by competition, and entered the Free Church Divinity College in 1858 with a view to becoming a minister. But about this time his attention was directed to medical missions, and he resolved to qualify himself to take part in them. To a man who had no resources and no friends the four additional years of study, which were requisite, were a serious matter. But he braced himself for the struggle. Again he taught in the academy, received private pupils, stitched the "uppers" of boots and shoes, and pored over his books. In July, 1862, he passed his second professional examination with much credit. At an early stage of his studies he had heard of the Edinburgh Medical Missionary Society, and it would have been a great advantage to him if he could have joined their institution, and thus obtained his education free of expense; but his family circumstances tied him to Aberdeen. He was quite alive to the benefit which he would derive from studying—if it were only for a time—under such men as Syme, Simpson, and Miller. He, therefore, determined to go up to Edinburgh for the last year of his course; and in 1862 he became an inmate of the Medical Missionary Dispensary. He distinguished himself greatly in his classes; but, notwithstanding, when he returned to Aberdeen to take his degree, he was plucked in those very subjects for which in Edinburgh he had received a gold medal and a certificate of merit. Judging — and perhaps not incorrectly — that the authorities at Aberdeen looked with disapproval on his removal to Edinburgh, he decided on leaving altogether the former university and graduating at the latter. But this involved an extra year of study, and again threw him upon his own resources; for he honorably declined to allow the expense incurred by his failure to fall upon the Medical Missionary Society. This year was the time of his hardest struggle, for he had to earn his own living at the time when he was reading more diligently than

ever. But he was rewarded with success; and he finally received his degree in 1864, his examiners being abundantly satisfied with the excellence of his papers.

A few months previously he had accepted the post of medical missionary in Kashmir under the Church Missionary Society; and he sailed for India in September of the same year. The first winter was spent at Lahore, where he worked hard at the language, and spent three or four hours daily in the government hospital in order to become familiar with Indian complaints. He reached Kashmir early in the following May, and opened his dispensary a few days after his arrival. Before the close of the first month he had as many as fifty patients a day, and had performed several important operations. The number of his patients steadily increased, but in September he was obliged to leave the valley, in accordance with the rule which was then in force, that no European should remain in Kashmir during the winter months. This law continued in operation during the seven years of Dr. Elmslie's missionary life. He returned to his post in the spring, and left again every autumn, spending the winters, for the most part, in similar work at Umritsar. His work increased upon him till at length there were often as many as 170 patients in a day. When it is remembered that he had no qualified assistant, and that even when performing operations and administering anæsthetics he could only have such help as a young native, whom he was training, could give him, the arduous nature of his duties may be appreciated. The native doctors, even if they had been ready to co-operate with him, could have afforded no assistance. They are utterly unacquainted with anatomy; the superstition of the country forbidding all *post-mortem* examinations. An idea may be formed of their ignorance by the following incident. "The Maharajah's native doctor being annoyed that most of his patients were leaving him for the mission dispensary on account of the superior surgery, in an evil hour for himself and his patient thought he would try his hand at surgery. He proceeded to open a boil in the groin of a sepoy; in doing so he cut into the femoral artery, and his unfortunate patient bled to death." More than once during Dr. Elmslie's residence in Kashmir the valley was visited by the cholera. The poverty, filth, and immorality which abound there were favorable to its development, and its ravages were fearful. Dr. Elmslie was indefatigable in his labours, going in and out amongst the people, and visiting from twenty-five to thirty cholera cases a day. In this emergency he was even admitted to see the women. Formerly, when in rare cases he had been called upon to give advice to native ladies, he was obliged to form his diagnosis with a

thick veil between him and his patient, through a hole in which he examined the tongue. Now, in their dire extremity, this custom of the country was not enforced. The hakims confessed that they were powerless, and the chief confidence of the people, from the Rajah downwards, was placed in the wearing of amulets and charms.

After he had spent five years in his medical mission work Dr. Elmslie returned to this country. During his residence in Kashmir he had, besides his professional labours, endeavoured to reduce the language to writing—a thing which had never before been attempted, and his chief employment whilst at home was passing through the press a Kasmiri vocabulary and dictionary. He worked so hard at this self-imposed task that he brought on an illness from the effects of which he never entirely recovered. The work was not out of the hands of the publishers till Dr. Elmslie had returned to India; and a completed copy, intended for him, reached Umritsar the day after his death. Whilst at home several appointments of value were offered to him, but he declined them all to return to his mission work. On the 23rd of February, 1872, he was married, and on the 5th of March he and his bride left Edinburgh for India. The succeeding summer, which he spent in Kashmir, was a particularly trying one. At the end of the first month it is noted, "he has just had to-day his eleven hundredth patient and finished his seventieth operation in a month." There also occurred, in August of this year, one of those inundations which are frequent in the valley. There was a great deal of general sickness at the time, and an epidemic of cholera followed. Besides the cases seen at the dispensary 382 cholera patients were visited in their own homes. It is no wonder, then, that the close of the season found Dr. Elmslie quite exhausted from overwork, and more fit for repose than for an arduous journey across the passes of the Himalayas. Repeated requests were made, both to the Maharajah and to the Governor of the Punjab, that he might be allowed, at least, to delay his journey. A special application was also forwarded to the Supreme Government at Calcutta, and the request that he might be permitted to winter in Kashmir was at last granted. But the necessary permit only reached Mrs. Elmslie the day after her husband's death. He left Kashmir the last week in September. The journey across the snowy mountains, at a height of 11,900 feet, was one of fearful suffering to a man whose heart and lungs were both seriously affected; the rough jolting of the "dandy" by day and the imperfect shelter at night aggravated his sufferings. He had no companion but his young wife, who was nearly exhausted by fatigue and want of sleep. His symptoms

became more and more urgent, and just as they had arrived at Goojerat, and were within reach of friends and medical assistance, he died.

Thus was a valuable life sacrificed to an absurd rule, which has now happily been abolished. The mission, however, has not been given up. It is now conducted by Dr. Theodore Maxwell. The Maharajah has built a hospital, and it is hoped that the work will in future be carried on throughout the year without interruption.

The Society for the Propagation of the Gospel in Foreign Parts carries on a college in the West Indies in conformity with the terms of General Codrington's bequest, to which we have already alluded; but the medical training given to the students seems to be altogether subordinated to the teaching in divinity. Indeed this society has done but little in the way of employing medical missionaries. It can point to two bishops—Dr. McDougall, late of Labuan, and Dr. Callaway, of Kaffraria—who were both actively engaged as medical practitioners before they became clergymen, and who have found their medical knowledge of no small use to them in their distant dioceses. Mr. Strachan, of Madras, who is an agent of this Society, is also a qualified medical man. But with these exceptions it can scarcely be said that the Propagation Society has taken any part in the recent development of medical missions which forms the subject of this paper.

The Church Missionary Society maintains an opium refuge at Hangchow, of which Dr. Galt is the medical superintendent. Dr. Theodore Maxwell, of Kashmir, is an agent of the same Society; and it has just sent out Mr. E. W. Forster to East Africa. The Rev. W. H. Collins, of Peking, is also a member of the Royal College of Surgeons.

The London Missionary Society has made more use of our profession than any other. It supports at present Dr. Dudgeon at Peking; Mr. Kenneth McKenzie at Hankow; Mr. G. W. Parker at Fianarantsoa, Madagascar; Dr. T. S. Thomson at Neyoor, Travancore; and Dr. G. A. Turner at Upolu, Samoa, South Seas.

The various Presbyterian churches, both north and south of the Tweed, have shown that the same zeal which led, in the first instance, to the formation of the medical missionary dispensaries in Edinburgh, leads them also to send medical missionaries abroad. At their foreign stations there are something like a dozen fully qualified medical men.

It has been a common practice with all the missionary societies to encourage their clerical agents to acquire such medical knowledge as they can pick up by the way. Thus, the students

of St. Augustine's College, Canterbury, attend the hospital regularly during their last year of training, and receive instruction from one of the physicians. Moreover, the societies have often established dispensaries and hospitals at their foreign stations, and engaged some local practitioner to attend to the patients. This is all very well, and these plans have, no doubt, been the means of alleviating a great deal of distress, and introducing among the natives more correct ideas with regard to the care and the nursing of the sick; but neither of these methods of proceeding fall within the scope of our present paper. We confine ourselves to medical missions as they are carried out by those who devote their whole time and energies to them, and who bring to the work all the knowledge and skill of highly educated and fully qualified practitioners.

If it is allowed that we confer a great boon by carrying the advantages of European medical knowledge to those nations that have no medical system at all, or only one which is worse than useless, a little reflection will show that it is not enough to send *medical men* to India and the East. This would but half supply the need. It must be remembered that there the women and children are secluded in the Zenanas, and that even the native hakims are seldom admitted to see them unless the patient is *in extremis*. The treatment of the native doctors is not only ignorant, but also excessively meddlesome, and calculated to do irretrievable mischief, when they *are* called in; but as a rule the females of India are in their times of sickness given over to the care of native nurses whose practice is even more harmful than that of the doctors. Very sad effects often follow from their gross ignorance and unlimited meddlesomeness. The death-rate among Indian women and children is enormous, quite out of due proportion. Dr. Elmslie says, "on account of the social habits of the people medical aid, to be extensively accepted, must be given by women. They alone, whether they be natives or Europeans, have free access to the Zenanas. Native gentlemen would only be too glad to call in even a European missionary lady, possessed of the necessary medical knowledge, to see a beloved wife in the hour of her trial, or a darling sick child, the pride and hope of their home. . . . . Western medical and surgical skill is esteemed everywhere, is everywhere greatly needed, and, we believe, would be everywhere readily and thankfully received by the women of India, if offered to them in a manner harmonising with the social customs of their country." Such considerations as these have led to the employment of missionary nurses by several of our societies—such nurses have generally received more or less training in medicine, more particularly in midwifery. For

instance, in Delhi there has been for eight years past a missionary accoucheuse, supported by the Delhi Female Medical Mission, who was trained in Europe, and who worked in connection with the Society for the Propagation of the Gospel. Her services have been so much appreciated that the work has extended. A dispensary for women and children has been opened, which is at present presided over by a Kaiserwerth deaconness, who not only attends to the patients, but is also training a class of seventeen native nurses. The benefit thus conferred upon the women of India has been recognised by the municipality of Delhi, a body largely composed of Mahomedan and Hindoo gentlemen, which grants £90 a year in scholarships to the native women who are thus trained; while the Punjab government contributes 410 rupees a year for medicine. This is a specimen of what is being done in different parts of India by our own country women, while the Americans have gone a step further, having sent out fully qualified medical ladies to act as missionaries. One such is stationed at Bareilly, another at Umritsar, and others elsewhere. These ladies not only train nurses to act under them, but are endeavouring also to raise up a class of native female practitioners. Whatever may be the feeling with regard to such practitioners in this country, there can scarcely be a difference of opinion as to the advisability of encouraging such a class to meet the wants of the female part of the population of our Indian empire. To the missionary societies is due the credit of having taken the first steps in this direction, and thus giving the earliest impulse to a movement which must have a most important bearing upon the sanitary condition and domestic management of the people; while at the same time it cannot fail to raise the social status of Indian women, and to emancipate them from the thraldom in which they are now kept. We were glad to observe in the *Madras Mail*, of the 11th of March, that the attention of Government has been given to the same subject. It is desirous of affording every encouragement to ladies to study for a degree in medicine. It has been decided that in order to do so they must attend the full curriculum of prescribed studies, with certain specified exceptions, these exceptions being midwifery, surgery, and one or two lectures in anatomy and physiology. For instructing them in these subjects special arrangements have been made; and as a further encouragement it has been determined that, for the present, no fees shall be required from lady students.

The amount of work, in a single department of practice, which the medical men attached to foreign missionary stations may have to perform is well illustrated by the following brief extract from the last report of the Medical Missionary Society

in China (1874)—a report to which we have already referred, and which is in every way most creditable. We there read that at Canton, and the five affiliated dispensaries

"There were thirty-eight operations for stone by lithotomy, with four deaths, and eight by lithotrity, all which were successful. Three operations for elephantiasis of the scrotum were performed with successful results. Extraction of the crystalline lens for cataract has been performed fifty-three times, and with restoration of sight in all but four cases. In addition to the above, other operations, amounting in numbers to more than a thousand, have been performed, giving partial or temporary relief to some, and effecting permanent cures in many. Altogether the year's work may be considered satisfactory."

In the same Report Mr. J. Vacken, who has a dispensary at Fuk-wing, says:

"I beg to offer my acknowledgments to Mr. Koffer, of the Medical Hall, Hong Kong, for the gratuitous supply of one lb. of the root of a Siamese plant, the name of which seems not yet to be known. The extract of this root yields a specific remedy for the cure of ringworm, which disease is very prevalent here. This medicine has proved unfailing in all cases, and therefore this root ought to become more generally known, and to find its deserved place in modern pharmacy."

Is this plant known to our dermatologists? If not, it would be worth while to inquire what it is, for the introduction of new medicines is one of the benefits which we may expect from the enterprise of medical missionaries.

It would be easy to multiply quotations from the reports of the different missions, giving particulars most interesting in a medical point of view—for example, of leprosy and its treatment; of opium smoking and its effects; of poisoning from eating the roe of the torpedo; of alcoholism in Mongolia, from the use of arak made from mares' milk; of a peculiar form of epithelioma met with in Kashmir, depending upon the habits of the people; of the successful introduction of vaccination in various remote regions; as well as of many other remarkable medical and surgical cases. But space forbids. We can, however, assure our readers that if they refer to the reports themselves, they will find a mass of interesting medical and surgical details which will well repay perusal.

The medical missionary societies are well aware of the fact, that if a country is to be properly supplied with doctors, it can only be by training natives. Their attention is, therefore, always more or less directed to this object.

There cannot be a better example of what may be effected by medical missions in developing a native school of medicine,

and substituting an enlightened system of treatment for the absurd and often harmful practices of the ignorant medicine-men of heathen countries, than what has occurred in Madagascar within the last fifteen years. Dr. Andrew Davidson, a pupil of Dr. Burns Thomson's, was sent to Antananarivo, the capital of that island, by the London Missionary Society in 1862. He opened a dispensary the first week of his arrival. His patients during the first year amounted to between 5000 and 6000. His successful treatment of the native prime minister, who had been a martyr to gout for nine years, procured him the favour of that officer, who built a house for the doctor, and granted him premises for a larger dispensary. He was shortly afterwards chosen Court physician, and received the medal of the order of Radama for his successful treatment of the king's son. During the political troubles through which the island soon afterwards passed the French physicians were obliged to leave, and Dr. Davidson remained alone, the one European medical man in an island with a population of fully 7,000,000. This gave him great opportunities, and he was not slow to avail himself of them. In January 1864 the foundations of a hospital were laid. The people entered heartily into the work. The nobles contributed largely to the expenses of the building. The Queen sent officers, with music, &c., to represent her on the occasion of laying the foundation stone, and expressed her sense of the value of the institution by desiring that it should be called the "Royal Hospital." Dr. Davidson's next step was to start a medical school in connection with the hospital, and Dr. Mackie was sent out to assist him. These two set themselves diligently to prepare text-books for the use of the students, feeling that if any good was to be done of a permanent and widespread character it must be by training native doctors. They appear to have found them apt students, and several are already taking the charge of outlying missionary dispensaries. A trained nurse was some years ago sent out to the "Royal Hospital," and she has a large class of native women under her instructions. From such a nucleus as this how much may be expected in the future for the amelioration of the physical condition of the Malagasy! Dr. Davidson is literally introducing to a whole kingdom and nation the blessings of European medical knowlege in the place of the grossest and most superstitious practice; and his training college bids fair to be not merely a medical school, but also the first step towards a national university. This is the medical mission which derives its pecuniary support chiefly through Dr. Burns Thomson.

The space at our disposal will not allow us to enlarge upon what has been done by the medical missionaries of other

countries. We have been obliged to confine our attention almost entirely to the work which has been accomplished by men sent out from this country. We must, however, remark, in passing, that the American Board of Missions have shown themselves fully alive to the value of this agency, and have sent out many most useful medical missionaries. Among the most remarkable of these was Dr. Asahel Grant, who laboured chiefly among the Nestorians of Persia (1836), and who, at a time when the life of a foreigner was scarcely safe in that country, traversed its mountain passes in perfect security, his only weapon being the cataract needle ; such was the reputation that he had gained by the successful use of this little instrument. In reading the reports of our own medical missionaries, especially those in India and China, we come across frequent mention of American medical missionaries who are carrying on the same work in adjacent stations. And, as we might expect from the greater prominence which has been given in the United States to the medical education of women, we find that they have not only sent out nurses, but also, as we have already mentioned, they have been the first to send out fully qualified lady doctors, to extend to the secluded inmates of Eastern Zenanas the benefits of a rational system of medical treatment.

To some of our readers the subject of medical missions may be altogether new ; while others may, perhaps, derive from this paper a clearer and more systematic idea of their scope than they have previously had. But to all alike it must, we think, be obvious that they are rapidly gaining in public opinion. They are in accordance with the spirit of the age, inasmuch as they are an attempt to spread to other nations some of the benefits of European civilisation, and we have no doubt that in years to come we shall hear more and more of their progress. Such has been their development in our own day, so numerous are the lines upon which they are advancing, so various are their adaptation to the diverse needs of humanity, that, having once made a fair start, they can hardly fail to win a large measure of public support and to advance with accelerated speed. Already the demand for suitable and well-qualified men is greater than the supply. At the present time the Edinburgh Society has before it ten applications which it is unable to meet. A new call is being made upon our profession. New paths are opening up before our students—paths which, if they do not lead to worldly advantages, are yet replete with scientific interest, and are particularly fitted to inspire the ardour of high-minded and generous men.

V.—The Teaching of Histology.[1]

For some years past there has been a cry for "practical instruction" in all departments of medical study. This cry is not of yesterday, though it is louder now than it has been for a generation. There was a time when systematic lectures formed the only recognised channel by which anatomy, chemistry, nay, even medicine and surgery, found their way to the average practitioner during his student-days. Steadily, and not very slowly, when the obstacles to such reform are taken into account, the lecture system in all these branches of the medical curriculum has been supplemented, first by an organised system of demonstrations, secondly by measures to enable each individual learner to acquire an immediate personal familiarity with a part or the whole of every subject taught. Physiology has been the last to yield to the stream of change; but it too has yielded, and a course of "experimental demonstrations," with a class of "practical physiology," now figure in the prospectus of every medical school which wishes itself to be thought on a level with the demands of the age. The recent regulations of the licensing bodies, making some such instruction compulsory on all candidates presenting themselves for examination, have powerfully contributed to hasten the progress of this fundamental change. In its latest phase the movement is no less than revolutionary; some of the advocates of "practical instruction" desiring to see lectures wholly abolished, as a mere waste of time. The text-book is to supersede the professor, and the student is to give up all his hours to the dissecting-room, the laboratory, and the hospital ward.

It may be worth while briefly to consider the logical issue of this tendency in its more general relations, before proceeding to discuss its bearing on the teaching of histology. It should be clearly understood that the change from simple lecturing to lecturing with demonstration, is different in kind from that involved in the substitution of "practical instruction" for professorial teaching. The former is the legitimate development of a method consecrated by long usage; the latter is a revolution. Whether a teacher appeal solely to the ear, as in lecturing, or to the eye together with the ear, as in demonstrating, he is still putting the stamp of his individuality upon a material more or

[1] 1. *The Histology and Histo-Chemistry of Man.* By Prof. Heinrich Frey. Translated from the fourth German edition by Arthur E. J. Barker, Surgeon to the City of Dublin Hospital. London, 1874.

2. *Handbook for the Physiological Laboratory.* Edited by Dr. Burdon Sanderson. Part I.—*Histology*, by Dr. E. Klein, Assistant Professor at the Brown Institution. London, 1873.

less plastic; the student still receives what the teacher has to give. But the scheme favoured by the most advanced school of reformers tends to substitute personal initiative for passive receptivity as a means of acquiring knowledge. The student is to teach himself; he is to be provided with all needful books and appliances and left to make his own way, the office of the teacher sinking to that of a convenient dictionary to be consulted at the learner's option.

This is not the place for a discussion of the absolute merits or defects of a proposal which we have designedly sketched out in its most extreme form. There is much to be said in its favour, when the end in view is to produce original investigators of a high order. Granted exceptional capacity, unlimited time, a limited sphere of effort, and an intention to penetrate into the domain of the unknown,—the best results might fairly be anticipated from a scheme under which no germ of latent power could fail of finding its appropriate soil and the requisite medium for its expansion. But are these preliminary conditions fulfilled in the case with which we are now concerned? It is sufficiently obvious that they are not. Ardent reformers in this department do not always appear to realise the immense extent of the field over which the mind of the medical student is expected to range during the four, or at most five, years of his pupilage. No other profession makes anything like the same demand upon the brain. The haughty boast, "I have taken all knowledge to be my province," becomes little more than a statement of fact in the mouth of a candidate for the London University degree. Now, an encyclopædic training of this sort can only be carried out in a limited time, on the condition that whatever independent powers the learner may possess shall be husbanded with the utmost economy, and turned into the narrow channel of strictly technical acquirement. They must be concentrated on medicine, surgery, and obstetrics, where every detail is of immediate value, and where the question of time occupies a relatively subordinate position, since the whole of the student's after-life is given up to these subjects. As regards the vast substructure of the physical sciences, it should be clearly admitted that an understanding of their leading principles is all that can be expected or desired, and that the greatest saving of time and labour may be achieved by allowing the student to remain a passive recipient. Systematic lectures and demonstrations must occupy the foremost place; they may be supplemented by "practical instruction," strictly limited to such points as are of intrinsic utility, or lend vividness and interest to the theoretical teaching.

In illustration of these principles we may take anatomy and

chemistry, two subjects in which the "practical" method has been followed for some considerable time. In the case of the former, besides attending lectures and demonstrations, the student has to dissect. Dissection—individual research—is an indispensable feature of anatomical instruction for two reasons: first, because it forms the only introduction to operative surgery; secondly, because without it the multitudinous facts of descriptive anatomy, appealing almost exclusively to the memory and hardly at all to the reasoning faculties, could not be adequately imprinted on the learner's mind. The same method is followed in the teaching of chemistry, but no longer under the same conditions. The theoretic foundations of this science are not only capable of being taught in systematic lectures, but they can hardly be taught in any other way. The experimental facts on which the laws of chemistry are based can be demonstrated by a skilful teacher to a large class of students, while any attempt to make the student work them out for himself would prove futile. Scientific chemistry rests on *quantitative* analysis. The course of "practical chemistry" attended by medical students consists chiefly, if not entirely, of *qualitative* analysis; and there is no prospect of immediate usefulness to set against this theoretical deficiency. Chemical analysis stands in no such intimate relation to any branch of medical practice as that which subsists between practical anatomy and operative surgery. The simple processes employed by the physician in examining urine, &c., might easily be learned at a subsequent stage of the student's career, without any of the elaborate machinery which has to be set in motion under the existing system, and, in point of fact, they are thus learned. The *reductio ad absurdum* of the present arrangements is to be found in the frequent inability of a student, who has gone through a course of practical chemistry, to ascertain the presence of chlorides in urine. The truth is that, notwithstanding the excellent method which is now pursued, and the conscientious zeal of very able teachers, the average student fails to carry away or to retain any real familiarity with even the commonest processes of analysis. He has wasted a good deal of time, and a quantity of expensive reagents; and the result—except in a few cases—is ridiculously out of proportion to the means employed.

If we look at the teaching of histology from the same point of view, we cannot but perceive that the students of this subject fall naturally under two distinct categories. It may be difficult, or even impossible, to distinguish at first between the individuals belonging to the one and those belonging to the other. The majority, in any class of medical students, purpose to devote their lives to practice; for them the results of histological re-

search, in their bearing upon general anatomy and pathology, are the main desideratum; they want results, not methods; or only so much of method as shall give a certain life and reality to the study of results. A minority, on the other hand, and in our English schools a very small minority, consists of men who will devote themselves to the prosecution of research. It is clear that for the former and larger group, who desire to obtain a maximum of information in a minimum of time, a dogmatic method and systematic completeness of exposition are required; the subject should be treated as a whole, and with a nice discrimination of the relative importance of its parts. Vexed questions, problems awaiting a solution, should be left, as much as possible, on one side. For the smaller number, an opposite mode of instruction is more profitable; their attention ought to be turned to the doubtful and uncertain borderland in which adventure may be crowned with discovery. For them it is an intellectual hindrance to view the subject as complete.

In our medical schools, at least for the present, the interests of the minority must be postponed to those of the majority. The minority must seek what it requires in private laboratories or abroad. We have to deal with a class of learners for whom systematic method is the prime requisite, both as a surer basis for the superstructure of professional attainment, and because it effects the greatest saving of time and labour. It behoves us, therefore, to consider *in limine* what it is that we desire to teach under the name of Histology.

The attempts of such men as Leeuwenhoek and Ruysch to unravel the minutiæ of structure, though productive of much valuable knowledge on special points, failed to impress any distinctive tendency on scientific thought. The true foundations of histology were laid by Bichat's great work on general anatomy, at the beginning of this century. They were laid, singularly enough, with hardly any reference to structural details beyond the range of the unaided eye. It was not until the cell-theory, in the original form given to it by Schwann, had furnished a scheme capable of embracing and co-ordinating the multitude of isolated facts which came pouring in from all quarters, that histology, in the sense in which we now understand the word, was definitely constituted. Our conceptions of the cell-theory have been greatly modified during the last thirty years; but it still remains the only framework by which the countless facts of tissue-structure, whether normal or morbid, are held together and rendered capable of being assimilated by the mind.

The word histology is constantly used to denote two perfectly distinct conceptions, and this loose and ambiguous employment

of the term breeds endless confusion. On the one hand, its etymology would naturally incline us to apply it to that branch of science which concerns itself with those simpler elements of which the more complex organs of the body are built up; on the other hand, the services rendered to that branch of science by the microscope are so many and so great, that the means have almost come to overshadow the end, and a term primarily suited to denote an intellectual conception has come to be frequently applied to a method of research. In other words, histology is sometimes made synonymous with general anatomy, and sometimes with microscopic anatomy. Now, the former is an abstract conception, and was arrived at, as we have already noticed, without the aid of the microscope; the latter is not a branch of science, but the fruit of an artifice by which our field of vision is enlarged. General anatomy stands in much the same relation to descriptive anatomy, as mathematical astronomy to the simple observation of the heavenly bodies; the former is not necessarily more dependent on the microscope than the latter is upon the telescope, though these instruments are indispensable for their prosecution; and any confusion of histology with microscopy, any attempt to place the method of observation on a par with the science to which it happens to be auxiliary, ought to appear as unphilosophical to the histologist as corresponding language in reference to the telescope would appear to the astronomer. It follows, moreover, that we ought not to fall into the mistake of looking to a more minute analysis of structure as the road to progress in histology. Supposing it were possible—a possibility nothing less than chimerical—to push our magnifying powers so far as to enable us to discern the chemical structure of the matter subjected to our instruments, to see the molecules in their rhythmic dance with the bodily eye, as we now see them with the eye of the imagination, we should not thereby enlarge the boundaries of general anatomy; we should simply penetrate by a new path into the domain of physics or of chemistry. The scientific completion of general anatomy must be worked out by the study of development. When once we are able to trace all the steps by which a particle of bioplasm is multiplied and differentiated into the complex variety of parts of which the adult organism is made up, we shall have done enough for our purpose. All questions as to the nature of bioplasm and the source of its singular endowments, lie outside the limits of morphology, and must be investigated by way of experiment, not by that of observation alone.

Histology admits of being taught in a course of systematic lectures illustrated by diagrams and specimens, both naked-eye

and microscopic. Strictly speaking, it ought not to be separated from anatomy, normal or morbid, of which it forms a part; it is only by accident that it has come to be associated with physiology, and the connection has not been a happy one for the latter subject. For many years, indeed, the teaching of physiology in our medical schools was fairly crushed by the unnatural usurpations of its yoke-fellow.

The course of "practical instruction" which is now all but universally added to lectures on histology, includes many things which do not properly belong to that science. It might, with greater justice, be termed a course of instruction in the use of the microscope for purposes of anatomical and physiological research. The two books whose titles head the present article illustrate the "systematic" and the "practical" modes of approaching the subject respectively. One appeals primarily to the larger group of students referred to above, the other to the smaller one. Neither of them is absolutely limited to histology, strictly so called, for Professor Frey devotes sixty-two pages to an account of the chemical composition of the tissues before proceeding to describe their structure, while Dr. Klein includes an account of various applications of the microscope to physiological experimentation.

Frey's work may be regarded as the legitimate successor of Kölliker's 'Manual of Human Microscopic Anatomy,' which it very much resembles in scope. Compared with the latest edition of Kölliker's manual, however (that published in 1860), it presents two obvious advantages. In the first place, it has been rendered into clear and readable English; in the second, it avoids any elaborate discussion of those questions on which the most eminent European histologists, for the present, agree to differ; questions interesting to the specialist, but which leave the ordinary student utterly bewildered—in that state of mind which was humorously alluded to some years ago by Professor Huxley, when he said that among the candidates in physiology at the University of London he found men disposed, when asked whether the blood circulates, to answer that "Professor Langkopf was of opinion that it did, while Professor Breitkopf believed that it did not."

The first part of Frey's manual is devoted, as already stated, to an account of the "elements of composition" of the body—in other words, to a brief sketch of its proximate chemical constituents. This is done simply, and without that display of graphic formulæ which meets the student in some modern text-books. The next part contains a full description of the simple and compound tissues; while in the third, the circulatory, respiratory, digestive, urinary, generative, bony, muscular,

nervous, and sensory organs are dealt with *seriatim.* The whole is abundantly illustrated by upwards of six hundred woodcuts, many of which are decidedly coarse, but their number may be allowed to compensate for any shortcomings of artistic quality. The entire work is characterised by a tone of robust and orderly common sense, and no part of it is allowed to usurp an undue predominance over the rest. In these points it is a model of what a student's text-book ought to be.

Of the changes which have been wrought in pure histology during the last fifteen years, and which render a new text-book indispensable, some idea may be formed by comparing Frey's chapters on the lymphatic system, and on the minute structure of the great nerve-centres, with those in Kölliker's manual. The progress has been essentially due to the employment of new methods of preparation.

As regards the lymphatic system, it may be truly said that we were ignorant alike of its glandular and of its canalicular portion. The labours of Frey and Teichmann, of Recklinghausen and His, of Ludwig and his pupils, of Sanderson and Klein, have cleared up most of the difficulties by which the subject was surrounded, and furnished a secure basis for inquiring into its functional and pathological relations. Injection by simple puncture (Hyrtl's method) has proved to be an easy and effectual way of eluding the opposition of the valves by which the larger lymphatic channels are guarded, and of determining their general arrangement and distribution in the various organs of the body. In many instances, the result of such injections might remain ambiguous, but for the aid of the silvering process. The demonstration of a limiting layer of condensed connective tissue forming the proper wall of every channel, and capable of being resolved into a mosaic of nucleated areæ, suffices to meet the argument that the injected liquid has simply forced its way along interstitial crevices and fissures. The endothelial markings are so easily defined by the nitrate of silver, and so characteristic, that there is no longer any difficulty in tracing the most minute lymphatic canaliculi even without the aid of injections. The continuity of the great serous cavities of the thorax and abdomen with the lymphatic system by means of distinct stomata, (a continuity originally foreseen by Mascagni,) has also been established within the last few years, and will prove fertile in consequences. It is especially satisfactory, moreover, that the methods by which these facts have been ascertained are so simple and so constant in the results they yield, that they can be successfully employed even by beginners in the art of microscopic research.

The anatomical study of the cerebro-spinal axis is a much more intricate matter. Here, too, the progress which has been made is due, in the main, to the means we now possess of obtaining large sections in which the mutual relations of the elementary parts are preserved. To make a sufficiently thin section of the cortex cerebri, or spinal marrow, without dislocation, it is necessary to harden the pulpy tissues; but hardening involves opacity. This difficulty is now easily overcome; we know several agents by which tissues hardened in chromic acid, bichromate of potash, &c., can be rendered perfectly transparent; those most commonly employed being turpentine and oil of cloves. Sections treated in this way, and mounted in dammar or Canada balsam, are quite transparent, and show all the tissue-elements *in situ*. By plunging them into various staining fluids before they are mounted, a still more complete differentiation of elementary parts may be obtained, and the structure of the nervous centres becomes as demonstrable as that of the kidney or of bone. Moreover, by substituting the microtome for the ordinary razor or Valentin's knife it becomes easy to make any number of successive sections in parallel planes, so that a given portion of the spinal cord may be thoroughly explored without any breach of continuity. Finally, by means of the freezing microtome, we are able to make trustworthy sections of almost any organ without the expenditure of time and labour required for hardening, &c. By methods such as these, our knowledge of the intimate structure of the great nerve-centres, both in health and disease, has been rendered as precise as that which we possess concerning the remaining organs of the body. Lockhart Clarke, Luys, Meynert, and others, have succeeded in demonstrating the invariable association of definite and localised lesions of nerve-centres with some of the most obscure diseases of the nervous system, thus redeeming one of the waste-lands of pathology from the neglect to which its apparent inaccessibility had long condemned it.

Dr. Klein's part of the 'Handbook for the Physiological Laboratory' differs widely from Frey's manual both in its merits and defects. Every scrap of information as to the methods employed in histological research has its value when coming from an acknowledged master in the art of technical manipulation; and there is hardly a sentence which does not carry intrinsic evidence of its source in personal observation and experiment. Though professing to deal only with methods of investigation, the work contains much teaching of a dogmatic kind. The student is told what he will see as well as how he should set about seeing it. The number of engravings, too, is far greater than is strictly needed for the purpose of illustrating methods.

These engravings stand on quite a different level from those in Frey's book. They are all of them original, and are meant, not as semi-diagrammatic sketches to elucidate the statements in the text, but as faithful representations of particular objects. Some of them are very beautiful, and reflect the highest credit both on draughtsman and engraver. Indeed, if we put aside the exquisite woodcuts which adorn many of Dr. Beale's books and monographs, we have not much to be compared with them in our micrographic literature. It may almost be said that they are *too* good for the purpose for which they are intended. Minute fidelity in the representation of microscopic objects is perhaps of less importance to the beginner than it may seem, at first sight, to be. If the alternative lie between a small number of accurate and highly finished drawings of particular specimens, and an abundance of rough woodcuts which, without actually misrepresenting anything, omit many of the finer minutiæ of structure, the latter ought certainly to be preferred in an elementary text-book. It should be borne in mind that a drawing, however accurate, represents not so much the actual specimen under observation as the inference drawn from it by a skilled observer. The impression conveyed to the mind of an histologist by certain appearances is a complex product, resulting from the inspection of lines and points in a series of horizontal planes, and from the comparison of a great number of similar specimens. It is this impression which is really embodied in a drawing. Those who have had occasion to employ a skilled draughtsman, unaccustomed to the microscope, in delineating an object under a moderately high power, will have been struck with the difficulty he finds in seeing what appear to be most obvious features. He draws what he sees; and he sees only those lines and points which are in focus at the moment. For a like reason photography is never likely to prove an efficient substitute for drawing in microscopic work. The camera is unbiassed and draws no inferences; for that very reason it yields information of scanty value. Hence, too, the most perfect drawings can never quite take the place of personal observation in the teaching of histology; *some* practical work is indispensable, even for medical students. Diagrams or rough sketches are sometimes better than true pictures for elementary instruction, the absence of detail enabling the main features to be more readily seized by the pupil, while he is never allowed to fall into the error of supposing that he has found a substitute for the labour of studying the objects themselves.

There are some positive defects in Dr. Klein's work to which it is worth while to draw attention, since they are of a kind that may be remedied in future editions. These defects are

particularly objectionable in a laboratory handbook, intended for reference during the press of actual work, rather than for methodical perusal in the study. They may, for the most part, be attributed to carelessness or haste. It is by no means easy to find what one wants without a long and vexatious search, in which the general index affords but little help, while the arrangement of the sections in each chapter is, to say the least of it, arbitrary. To add to the reader's perplexity, the numbering of the sections, adopted throughout the remaining divisions of the handbook, is unaccountably omitted in this part of it. Moreover, the connection of the text with the engravings is often left to the reader's ingenuity; the latter (issued in a separate volume or atlas) are sometimes unprovided with any reference to the letter-press, or provided with a reference not specific enough to be of much service. The text ignores the illustrations even more than the illustrations ignore the text. Considering their rare excellence, it is somewhat annoying to find that out of 188 engravings only 30 are referred to in the body of the work. Had the text been produced by one author and the illustrations by another, their mutual independence could scarcely have been more vigorously asserted than it is. It is further to be regretted that the excellent plan recommended and invariably adhered to by Dr. Beale, of adding to each drawing the magnifying power in diameters, should not have found favour with Dr. Klein. The prevalent Continental custom of stating that a particular object was drawn under "Hartnack's oc. 4, obj. 7, tube half drawn out," is not only wanting in scientific precision, but is highly inconvenient, especially as the magnifying power of the various combinations of Hartnack's lenses is nowhere given, and their use, at any rate in this country, is far from being universal.

In its present state, therefore, this portion of the handbook is better suited to the wants of the advanced student and the original worker, than to those of the beginner. Where the end in view is to enable a number of medical students, of various degrees of ability and industry, to obtain ocular demonstration of facts which have been communicated to them by systematic lectures and text-books, clearness and simplicity of arrangement are of the first importance. Any obscurity of exposition, any disproportion between the amount of time and labour given to a particular question and the importance of that question relatively to the entire science, is sure to make itself disastrously felt in the confused and blundering impressions left on the mind of the average learner. The only scheme, so far as we are aware, under which an ordinary class can be guided through the entire subject during the limited time available for the pur-

pose, is that which was followed by Professor Rutherford during his tenure of office at King's College.[1]

The following quotations embody the leading features of his plan, and exhibit its eminently practical character:

"It is not advisable to teach more than five-and-twenty, or, at the outside, thirty students at a time; to teach even this number satisfactorily requires three skilled assistants—senior students who have been through the course and can assist the uninitiated. The whole of histology may be gone through in twenty-four lessons, each lesson lasting from an hour and a half to two hours.

"The plan I now adopt on all occasions is to treat my class like a regiment of soldiers, making every one do the same thing at the same time. The interest is thoroughly kept up by making the class a sort of debating society. While every one looks at the same objects, I ask one student after another to describe what he sees. The observations of one student are in this way checked by the observations of another. The class resembles a little army of investigators; it is, in fact, a miniature of the histological world. The power that every student has of criticising the observations of his fellows makes, as might be supposed, every one exceedingly careful. A simple object like yeast is taken first. I give no description of the torula, but I ask the student to describe and then to draw what he sees. Any one may be called on to do this. Any one who disagrees with any statement is asked to do so, and to give a demonstration in support of his opinion. To facilitate such descriptions every student has a card on which are printed the following points:—1, shape; 2, edge; 3, colour; 4, transparency; 5, contents; 6, size; 7, effects of reagents. The card prevents the student from getting bewildered, and teaches him method and thoroughness. Care is taken that no one ever becomes idle. If his preparation is made, and he be waiting on his neighbours, he occupies his time in drawing. When we come to complex structures, such as bone, I give a brief preliminary account of the subject, in order that every one may understand what he sees. By questioning the student as the demonstration goes on it is easily ascertained if he knows what he is about. On all occasions, however, I make the student describe what he sees. This method really *educates* him in a way such as no other method, in my opinion, can. When necessary I show preparations which have been previously made.

"At the beginning of the course a general account is given of the microscope and accessory apparatus. The student is instructed how to clean the instrument, how to measure its magnifying power, how to measure the size of an object, and how to draw. After this every student is provided with his microscope, and the regular work of demonstration begins. From time to time, as the course advances, short accounts are given of the preparation of objects, &c. These are not entered into, however, until the student has had a little experience of the effects of agents upon torulæ, blood-corpuscles, &c."

[1] Published in the 'Quarterly Journal of Microscopical Science,' Jan., 1872.

The arrangement of Dr. Klein's manual conforms more closely to that followed in systematic works on histology than would perhaps be found convenient in teaching a class. Of its two principal divisions, one is concerned with the preparation of the simple, the other with that of the compound tissues. The first chapter is devoted to the blood-corpuscles. The mode of observing the amœboid movements of the leucocytes in the blood of homœothermic and pœcilothermic animals respectively is fully described, as also the effect of gases, liquid reagents, electricity, heat, and moisture, on both red and colourless elements. The next chapter gives an account of epithelium and endothelium, including the study of ciliary motion and the employment of nitrate of silver. The third chapter describes the methods of preparing and examining the various connective tissues (fibrous, elastic, and areolar tissue, the cornea, different kinds of cartilage and bone). The fourth deals with striped and unstriped muscle, including Brücke's method of investigating the former by means of polarised light. In the fifth chapter we have an account of nerve-cells, nerve-fibres, and their terminal distribution. The second part opens with a description of various modes of cutting sections, hardening the compound tissues, embedding, staining, and mounting them. The different processes for injecting the circulatory system are then discussed, the discussion leading up to an account of the structure of blood-vessels, and the way to observe the circulation in the living organism. The next chapter is devoted to the lymphatic apparatus, and includes a description of the methods adopted in an investigation whose results have recently been published in the form of a monograph. This part is more lavishly illustrated than any of the others. The organs of respiration and digestion are dismissed in two relatively brief chapters, followed by an account of the skin and the genito-urinary apparatus. The organs of special sense come next in order. Lastly, we have a chapter on embryology which is too difficult of comprehension for the beginner, while it is too concise for the wants of the advanced student. Now that we are in possession of the admirable monograph by Foster and Balfour, this chapter may be regarded as superfluous. A short appendix gives some of the methods of studying the process of inflammation in epithelial structures, cartilage, the cornea and tongue of the frog, &c., methods which have been so fertile in valuable results during the last few years, but which belong rather to pathology than to normal histology.

Briefly to recapitulate. The teaching of histology to a class of medical students should, under existing circumstances, be rigidly systematised, and connected as closely as possible with

that of anatomy. Frey's manual is probably the best guide we have for this part of the course. The "practical instruction" by which lectures ought, in all cases, to be supplemented, must not be suffered to usurp their place, but should be carefully subordinated to them. Its principal requirements are order and method, if the interests of the majority of students are to be preferred to those of a small minority. Dr. Klein's treatise, in its present form, is better suited to the wants of the private student than to those of a mixed class; it contains, however, a mass of exceedingly valuable material which might easily be modelled, by a practised teacher, into a thoroughly workable scheme.

VI.—Diet in Health and Disease.[1]

WHAT to eat, drink, and avoid, is a branch of knowledge of no mean importance to our bodily and mental welfare. It is one, indeed, more or less cultivated, and yet most grievously neglected, by mortals generally. It is a constant subject of conversation and a matter upon which every individual considers himself more or less an authority, and upon which also he is most ready to give instruction to others. It is, moreover, a subject whereon each individual is apt to regard himself as the best judge relatively to himself, and yet is one about which the self-taught and self-satisfied individual is most prone to make a mistake, or to meddle with to his own detriment. Lastly, it is a branch of knowledge on which many books have been written; some of these have rather darkened knowledge, whilst others have embarrassed their readers by overmuch philosophy, borrowed from the chemist and physiologist, or have harassed them by one-sided theories of nutrition and impracticable rules of diet.

Nothing can be more foolish, and too often mischievous also, than the conceits and notions respecting diet prevailing among the public. Doctors are ever at work correcting those notions and counteracting their consequences, but their labour is too much like that of Sisyphus. There is a downward gravitation to folly and self-indulgence frustrating all their efforts. Their disciples are often prejudiced, and oftener unwilling, learners. They have to encounter a giant enemy to their dietetic rules in the shape of idiosyncrasy, a shadowy form, in truth, in many instances, but sufficiently formidable to effect their frequent discomfiture. And what is more inimical to their efforts, the doctors are much divided among themselves; they have favourite

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[1] *A Manual of Diet in Health and Disease.* By THOMAS KING CHAMBERS, M.D., &c. London, 1875.

crotchets, pet axioms, phantom fears; they follow fashion, or court singularity by opposing it; they lend themselves to extravagant doctrines and swear by stimulants or by teetotalism.

These remarks would seem tantamount to saying that the principles of dietetics imbibed by medical men are uncertain and without influence among them. In some degree this may be true, but it is not the whole truth. There are principles, based on well-determined truths in chemistry and physiology, and held in common by all instructed medical men; but there is likewise a mass of knowledge concerning food and drinks never systematically placed before medical men during their career as students. They pick up incidentally, here and there, maxims of living, as chance directs, wholesome or vapid, which cleave to them as long as they practise, with small modification. They adopt the current opinions of their time with reference to most points, and they find it hard to decide, particularly when called on to determine the dietary of disease, between the doctrines held by rival teachers.

It is with regard to the "dietetics of sickness" that concurrence of medical opinion is most called for, as well in the interests of the profession as of the public. It is a matter upon which the public look for general agreement; and it is, moreover, one regarding which we ought to be enabled to arrive at definite and established principles, inasmuch as it falls within the limits of experimental science. Unfortunately, however, experiment is little resorted to with regard to diet compared with what it is with reference to drugs, although diet is frequently as important as the latter. Whilst this great question is, therefore, undecided among the doctors, to whose particular province it appertains, no marvel is it to find a host of contradictory notions prevalent among their clients.

Agreeing as we do with Dr. Chambers that man is an omnivorous animal, small astonishment need be expressed at the diversities of opinion among mankind respecting the wholesomeness of various sorts of food. The normal people are those who know not they have stomachs; when the discovery is made much of their happiness is lost, and the question of diet crops up for solution, and their eyes are opened to the difference between good and bad. These normal people would abound on the earth, were not the conditions of healthy animal existence deranged and overturned by the habits, the fashions, and the exigencies of civilised life. From these the ills of the stomach originate, and each conscious possessor of that organ has ever before him the problem of what to eat, drink, and avoid, and among the multitude of edibles before him each man in the exer-

cise of free choice, though hampered by prejudices and fashion, selects those most pleasing to him, and which, too, he is prone to regard as also superior in digestibility and nutritive qualities to those approved by his neighbour.

It is well when he is content with the contemplation of his own discernment and does not wish to enforce his dietary upon his neighbour. It is well when charity aboundeth between him who eateth herbs and him who eateth meat, between the drinker of wine and the abstainer from it. Unhappily, as daily observation exhibits, such charity is largely displaced by intolerance, whilst liberty is denounced and its destruction demanded.

Any book that can correct popular misapprehensions about diet, that can set forth in plain language the accepted principles of dietetics, and expand our notions with reference to them and their application, and that can withal do this from a liberal point of view, untrammeled by party cries, and can for its groundwork appeal to ample experience and observation, is one that must be welcome to the masses to whom we have referred as so much in need of instruction. We have carefully examined the treatise before us, and can confidently recommend it as a book of the sort.

The author, Dr. King Chambers, is well known to the profession by previous publications of great value, particularly by their truly practical character. His volume of clinical lectures admirably illustrates the cogency and clearness both of his teaching and of his literary style. And the subject of his present book is one by no means newly taken in hand. In the volume of lectures already referred to, the dietetics of sickness occupied no mean space; whilst in his treatise on "the Indigestions" diet, as a matter of course, constituted a special topic for consideration. We may consequently say that the subject of diet is one he has peculiarly made his own, and the profession will on this account, even apart from his known high position as a metropolitan hospital physician and teacher, receive with much confidence this his latest production, 'A Manual of Diet in Health and Disease.'

But it is time to give some particulars of the contents of the book, although we do not propose to review them at great length: not but that he has supplied material available for the exercise of the critic's art, particularly in doctrines and statements respecting matters somewhat outside the subject, to discuss which, however, we do not esteem it our present business. For instance, we cannot concur in all the opinions conveyed in his chapter on "noxious trades," or in that on "athletic training." We are not so impressed with the demonstration of the harmlessness of boat-racing attempted by Dr. Morgan, as Dr. Chambers appears

to be; nor can we accept it as sound general doctrine that there is no actual increase of muscular tissue in a limb actively used, or, to use the author's words, "that they [the muscles] become larger is extremely problematical;" nor admit "that, if muscles did by training grow bigger, as reckoned in a state of repose, it were a result not at all to be desired;" nor, again, the assertion that hypertrophied muscular organs lose their shape and power; for the examples he quotes in illustration of it are taken from morbid states, and will not on examination bear out the dictum.

As just now remarked, such matters are somewhat outside the special subject of his treatise, and a captious critic could point to many paragraphs in respect of which exception might be taken as not sufficiently cognate with the subject-matter in hand. In short, the teacher of the practice of medicine and of pathology is here and there too prominent. His utterances are good, but do not truly belong to the subject of diet.

The work is divided into three parts—general dietetics, special dietetics of health, and dietetics in sickness. The first includes chapters on the theories of dietetics, on the choice of food, on the preparation of food, on digestion, and on nutrition. The chapter on the theories of dietetics is marked by much moderation in chemical and physiological detail, yet withal conveys a large amount of information respecting the essential characters and the purposes of food. This moderation in detail will be thankfully accepted by most of his readers, who, wanting to be instructed in dietetics, do not require a dissertation on the anatomy and physiology of the stomach, or on the chemistry of food in general. For, so far as medical men are concerned,—and to them such works as the one under notice are especially addressed,—it may be assumed that they possess a fair knowledge of the stomach and its functions, and of the chemistry of articles of diet in common use. Yet what is more common in medical literature than to find in special treatises, preliminary disquisitions on the anatomy and physiology of the organ or organs treated of pathologically? as though their medical readers had for the first time to be instructed in those matters. Such dissertations belong to the sort of literary lumber called "padding;" they are a nuisance to the reader seeking for enlightenment on questions important to him as a practitioner, and professional opinion should be pronounced against the practice of introducing them.

Treating on the "choice of food," Dr. Chambers notices *seriatim* the principal articles of consumption, including wines, making excellent practical remarks on their value as food, on their character when wholesome and genuine, and on the adulterations practised with regard to them. He discourses on the

ocular and tactile qualities of flesh, fish, and fowl like a well-instructed housekeeper, imparting lessons which it would be well that every housekeeper might obtain.

Wines he reduces to seven classes, viz.—1, strong dry wines; 2, strong sweet wines; 3, aromatic wines; 4, acid wines; 5, sparkling wines; 6, perfect wines; and 7, rough wines. Of these several sorts the reader will like best to know what are "perfect wines." These are defined to be—

"such as possess the virtues derived from the presence of alcohol, of water, of sugar, of etherial flavours, of fruity extractive, and of acids, without any of them being so predominant as to mask the others, or to require artificial additions for the preservation of soundness and flavour."

The example of such perfection is found in "claret"—not, indeed, in the fine-named varieties, such as Château Lafitte or Château Latour, but in the unnamed sorts known in the wine trade as "first, second, and third quality clarets"—the genuine fermented juice of the grape without admixture for the sake of special flavours, or, what is more objectionable, for the purpose of fortifying with spirit what is damaged or imperfect of its kind.

"Against the adulteration of claret [he adds] which does not pretend to be anything else than 'first,' 'second,' or 'third' quality, we have the valuable safeguard of the enormous quantity that is made, and the small profits which could be got out of the labour and risk of adulterating it."

Much truth undoubtedly exists in these remarks. They convey a wholesome lesson to the buyers of the fine-named wines so forcibly and persistently pressed upon us by interested advertising dealers, who seem of late years to have multiplied like locusts, and are well-nigh as unmitigated plagues. At the same time we should not entertain an equal feeling of security with Dr. Chambers against adulteration, if content to purchase nameless clarets on the ground that adulteration of such would not pay, for both in the country of their growth as well as in England it has been our lot to meet with vile stuff sold as claret without name, and beyond question sophisticated in no slight degree.

The author proceeds to observe that "the common Burgundies and the red Rhine wines run our claret very hard in the race for perfection; they err in containing too much fruity extractive, which, except the wine happens to be very strong in alcohol, causes decomposition." Beaujolais when good receives commendation, but the Hungarian wines are "damned with faint praise."

"On the preparation of food" the information conveyed has

the same practical and precise character as that respecting the choice of food. Dr. Chambers discourses on cookery with the air of a professor; likewise, when we reach the following chapter on digestion his teachings lose little of that decided and unhesitating or dogmatic character with which the previous pages of his treatise have made us familiar. This feature we notice, though not as a fault. On the contrary, it is a merit in a teacher who has got distinct opinions, based on study and experience, to offer, and a fortunate circumstance for his pupils. It is for the latter to sit at the feet of their master, to receive, but presently to "prove all things and hold fast that which is good."

Opinion has differed, and even still differs, respecting the desirability of an after-dinner nap. It is a matter, however, on which the author has made up his mind. Instinct, he admits, tells us to take rest after meals, but the rest must not be in excess, and "sleep, for example, after dinner retards digestion, and allows the distended stomach to act injuriously on the circulation of the brain. It is proper only for very aged persons or invalids." For though instinct prescribes rest, Dr. Chambers would not have us take for example "dogs and other carnivorous animals who betake themselves to sleep after a repast;" because, in his view, this instinct is a vicious one consequent on the bad habit such animals are apt to follow of unduly gorging themselves with food, especially when living in a wild state. For, as he further asserts, "dogs which live in the company of men, and feed on the mixed and cooked diet of their masters, usually give up the practice of sleeping after their meals along with their gluttony," and, as a reward of merit, suffer less from indigestion than their wild companions.

This teaching is admirably distinct and decided; but, we consider, it is to be received with modifications and exceptions. The old argument by analogy from the lower animals does not, we conceive, lose much of the validity it has been thought to possess by the objections urged against it; nor can the physiological arguments used against sleep be accepted in their entirety, or as so universally true as Dr. Chambers would have us believe. It may be difficult to prove, as the author remarks, that the wild animals who sleep after a feast have their lives prolonged thereby, but the proof that they are the worse for the sleep is equally not forthcoming. People who sleep after food are not in the habit of so doing except after their principal meal; and when that meal has been duly earned by labour, it will need some distinct evidence, something more than general physiological dicta, to convince us that a short siesta is detrimental to health. We have in view, not the lethargic sleep of pampered

and lazy people, but the apparently instinctive sleep of the labourer when time and opportunity are afforded him.

Part II of the volume is devoted to the "Special Dietetics of Health," and the instruction it contains is marked by the same good sense as other portions. It is also interspersed with incidental literary remarks and allusions, as elsewhere met with throughout the treatise, which render its perusal a pleasure as well as a profit. But there is little to detain us in its several chapters until we reach the last, viz. that on alcohol. To this article of diet and medicine Dr. Chambers has given very full consideration, and his observations on this 'inflammatory' matter are distinguished by moderation and judgment. In his chapter on the dietetics of literary life he has well summarised the properties of alcohol as a restorative, when taken with food, of nerve-power.

"The physiology of the action of alcohol [he writes] has a very practical bearing on the physical regimen of the mental functions. Alcohol has the power of curbing, arresting, and suspending all the phenomena connected with the nervous system. We feel its influence on our thoughts as soon as on any other part of the man. Sometimes it brings them more completely under our command, controls and steadies them ; sometimes it confuses and disconcerts them, then breaks off our power and the action of the senses altogether. The first effect is desirable, the others to be avoided. When a man has tired himself with intellectual exertion, a moderate quantity of alcohol taken with food acts as an anæsthetic, stays the wear of the system which is going on, and allows the nervous force to be diverted to the due digestion of the meal. But it must be followed by rest from mental labour, and is, in fact, a part of the same regimen which enforces rest—it is an artificial rest. To continue to labour and at the same time to take the anæsthetic is an inconsistency. It merely blunts the painful feeling of weariness, and prevents it from acting as a warning."

The popular notion, fostered by many a bacchanalian song, that wit and humour are evoked by alcoholic drinks, receives no encouragement from Dr. Chambers. "An abstainer [he says] in a party of even moderate topers finds their jokes dull and their anecdotes pointless, and his principal amusement consists in his observation of their curious bluntness to the absurdity of their merriment." But so far as this statement is worth anything, the topers might find another interpretation, and assert that the abstainer's wits are too dull to understand or appreciate their jokes and anecdotes, and need to be sharpened by good liquor. However this may be, we are not disposed to subscribe to the general denial of the quickening or brightening influence of alcohol on the mental powers. Observation both in olden and modern times is opposed to it.

To turn, however, to the special chapter on alcohol (one of the longest in the book), we find Dr. Chambers in the character of an original experimenter on the physical effects of that substance. His first experiment was made on a healthy man, from three to six ounces of brandy being given daily in portions with the four meals; the second, made on the same individual, consisted in giving six ounces in doses of half an ounce every hour; the third was carried out on a man used to more full living than the previous one, the brandy being an addition to his usual allowance of stimulants in the shape of from five to seven glasses of port or sherry. To these experiments is appended an observation made in the case of a young woman who had fallen into habits of excess. The conclusions gathered from the first experiment were, that alcohol is not a diuretic, but that it increases the formation of urea, and consequently provokes a more active destructive assimilation and a more active reconstruction of the nitrogenous elements of the tissues, and that accompanying this interstitial growth is a temporary rise of the digestive powers of the stomach; further, that the excretion of phosphorus is decreased, a circumstance arguing diminished chemical changes in the brain and nerves, with consequent lessened and blunted function, and lessened general vitality, as exhibited by decreased ability to generate heat.

Where food, he argues, is keenly enjoyed without recourse to stimulants, "their consumption is certainly useless, and possibly injurious. So long as alcohol, in the indirect mode mentioned, augments vital metamorphosis, it ministers to the force of the body. But it is not a source of force, and its direct action is an arrest of vitality." On the other hand, where the intellectual or muscular energy is borne down by pressure of work, there a fair ration of beer or wine at their evening meal is of material service. Most wisely and energetically does Dr. Chambers warn his readers against the drinking of spirituous liquors in the morning, and equally wisely and well does he pronounce against taking stimulants between meals.

His second experiment comes in support of these protests, by proving that alcohol, in small divided doses and between meals, "deranges the metamorphosis of the tissues, and in the direction of arrest; and persistence in the habit must lead in the end to permanently diminished organisation, degeneration, atrophy."

It were well could these doctrines be instilled into the rising generation of our commercial towns. We are pained to see our young men resorting to luncheon bars and taverns at all hours of the day between meals, to drink a glass of bitter beer,

or, not unfrequently, some *liqueur*. It is a growing vice, fruitful of evil, physical and moral—one that has been much encouraged by the comparatively modern institution, the "luncheon bar."

Towards the close of the chapter the author sums up, under thirteen heads, his conclusions relative to the influence of alcohol upon a healthy man, and deduces therefrom four rules and many corollaries as to the virtues and uses of that agent. We should be pleased to reproduce them here, but they are too long to be transferred to our waning space.

He looks for reformation in the abuse of alcoholic drinks to the habits and silent pressure of amended social life. Progressive improvement in our habits and customs must, he affirms, avail more than all repressive legislation; and to forward such improvement he holds to be a special duty of medical men. Special legislation in behalf of dipsomaniacs receives small encouragement.

"There is [says Dr. Chambers] a difficulty in sending a drunkard to an asylum, but he is easily sent to 'Coventry' and made to feel a degraded animal. There may possibly be some rare cases to be found of true 'dipsomania,' where, without any other mental disease, the patients are carried off by an uncontrollable impulse to drink; but they certainly are very rare indeed, and every alleged instance that I have investigated has always exhibited also some other form of insanity sufficient to justify the imposition of restraint, or else proved to be using the cant of the day as an excuse for self-indulgence. The exceptional cases may be separately dealt with when they occur; but as a rule I think it better to give men the education of being their own gaolers than to let them lean on the weak crutch of State inebriate asylums."

The third and last part of the book is occupied with "Dietetics in Sickness;"— including acute fevers, inflammatory states, weak digestion, gout and rheumatism, gravel, stone, diabetes and albuminuria, deficient evacuation, nerve disorders, scrofula, rickets and consumption, and disease of heart and arteries. To the medical man this section of the treatise will possess a special interest; it exhibits within a very brief compass the lessons gathered from a very long and large experience. However, as much of the teaching has heretofore appeared in other works of the same author with which the profession is familiar, it is not here necessary to produce it. It is, in our opinion, very sound and safe teaching—curt, decisive, and easily remembered, and presented in a clear and engaging manner. A companion portion is attached to several chapters, setting forth "recipes" for various articles of diet suitable to the sick, and some useful supplementary information is supplied with

regard to change of climate, particularly in the case of gout and rheumatism.

From the preceding remarks a fair notion may be gained of the matter and manner of this most recent English book on diet. A review of the whole work justifies us in strongly commending its perusal to all our readers.

VII.—Microscopic Organisms in Health and Disease.[1]

The series of memoirs whose titles we have enumerated below are all the fruit of the well-directed energies of the Sanitary Commission of the Government of India, and reflect as much credit on that organization, as do the valuable researches issued under Mr. Simon's direction, on our own Privy Council Medical Department. The subjects have been well selected, the researches ingenious and exhaustive; and success, which does not always wait upon experiment, has crowned the labours of the indefatigable investigators.

The inquiries with which we are now concerned have, in spite of their variety, a certain unity of aim, being all directed to investigate the influence of organized beings, animal or vegetable, on human life; and, as regards the vegetable organisms, with a special view to detect any influence of such organisms on cholera. It may not be superfluous to point out that Drs. Lewis and Cunningham are peculiarly fitted to pass a judgment on these points, since, besides their medical and biological training, they have worked under the great fungologist, De Bary, with the special object of becoming acquainted with microscopic vegetation. We may, therefore, conclude that they know, as far as may be known, what is and what is not a vegetable organism, which could hardly be asserted of all investigators who have attacked this difficult theme.

Their researches fall under three heads, which we will consider separately:—1. The microscopic examination of air.

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[1] 1. *Microscopic Examinations of Air.* By P. Douglas Cunningham, M.B., attached to Sanitary Commissioner with Government of India. (Not dated.)

2. *Report of Microscopical and Physiological Researches into the Nature of the Agent or Agents producing Cholera.* By T. R. Lewis, M.B., and P. D. Cunningham, M.B. Calcutta, 1872.

3. *Report on Cholera.* (Second Series.) By the same authors. Calcutta, 1874.

4. *On a Hæmatozoon inhabiting Human Blood, and its Relation to Chyluria and other Diseases.* By T. R. Lewis, M.B., Assistant-Surgeon, attached to Sanitary Commissioner with Government of India. Calcutta, 1872.

5. *The Pathological Significance of Nematode Hæmatozoa.* By T. R. Lewis, M.B. Calcutta, 1874.

2. Direct investigations on cholera. 3. Researches on entozoa, which are not immediately connected with the other topics.

I.—Microscopic Examination of Air.

Dr. Cunningham has contributed some important observations on atmospheric micrography, as it may be called, which are of importance, negatively, in the question as to the presence of germs of disease in the air. He first gives a review of the literature of the subject, which is very full and careful. The results of previous observers are characterized with justice as extremely contradictory, some affirming, others denying, the presence of organized bodies or their germs in the atmosphere. But, as it seems to us, the preponderance of number and authority is on the side of those observers who have found in very many cases the germs of fungi in dust or suspended in the atmosphere.

Dr. Cunningham's observations were made with the view of determining, if possible, whether there were any connection traceable between the prevalence of special bodies in the atmosphere and the occurrence of particular forms of disease. With this object the air of two large jails at Calcutta was examined on fifty-nine occasions by an apparatus used by Dr. Maddox in similar experiments, consisting of a sort of funnel, the open end of which was kept always exposed to the prevailing current of wind, by freely rotating on a spindle, and having a broad vane at the other end. The current of air was made to impinge upon a cover-glass smeared with glycerine, by which all dust was retained. The glass was allowed to remain for twenty-four hours, and then transferred to the stage of the microscope. The apparatus was placed about five feet from the ground on an open space of grass.

The solid matters thus collected are classified as follows:

1. Particles of siliceous matter.
2. Particles of carbonaceous matter.
3. Fragments of hair and other animal substances.
4. Fragments of cellular tissue of plants. Starch-corpuscles nearly always.
5. Pollen-grains, amongst which those of several common grasses could be easily recognised, and a few belonging to plants of other natural orders.
6. Algæ, but not in great numbers. Among these were fragments of Oscillatoriæ, Desmidiaceæ, &c., with some lower genera.
7. Sporidia of lichens were frequent.
8. Spores and sporidia of fungi, often at once referable to their proper genera, formed, in fact, by far the greater part of

the bodies found. The most common forms were sporidia of *Sphæriaceæ*, frequently in a state of germination, but names of numerous other families are given by Dr. Cunningham. He does not himself venture to affix generic names; but this has been done to a considerable extent by the Rev. J. M. Berkeley, in an article in the 'Quarterly Journal of Microscopical Science' for April, 1874, which we have made use of in estimating the value of Dr. Cunningham's researches. The judgment of this eminent botanist must be regarded as decisive of the true nature of the organisms. He observes that the extraordinary quantity of fungus-spores carried about by the air is very remarkable, and refers to the observations of Ehrenberg on the existence of fungus-spores in the dust of the trade-winds, thousands of miles from land (Ehrenberg, however, found in ordinary *dust* vegetable much less numerous than animal organisms, such as infusoria). Such observations obviously supply a perfectly adequate explanation of the apparently spontaneous growth of fungi when moisture and other circumstances are favorable.

Some miscellaneous observations, in addition to this regular series, remain to be considered. A certain number of specimens were collected specially with the view of ascertaining whether bodies definitely recognisable as bacteria occurred constantly or frequently in the air. In these observations there was a general absence of distinct bacteria, but, "as is almost always the case in atmospheric dust, a general diffusion of minute particles resembling those included by Robin under the term micrococcus," with respect to which "it is impossible to deny that many of them may be of a bacterial nature." Bacteria were, however, developed in abundance from dry dust, collected from various localities, such as the leaves of trees, even in the hottest and driest seasons of the year at Calcutta, when this was added to suitable fluids, such as urine, though the same fluid remained quite free from bacteria when no dust was added. Similar observations have, of course, been repeatedly made.

With respect to the composition of dust as commonly occurring in Calcutta, one interesting point brought out by numerous observations was the almost entire, if not entire, absence of infusoria, their cysts or ova; though they have been repeatedly observed (*e. g.* by Ehrenberg) as occurring in abundance. Perhaps the scorching heat and dryness of the Calcutta climate may explain this.

A series of observations was also made on the organisms in rainwater, intended chiefly to throw light on the question of the existence and diffusion of bacteria in the air. The result was that specimens of rainwater in Calcutta, collected with every precaution to ensure their freedom from contact-con-

tamination, frequently show sooner or later the presence of spores, mycelium, zoospores, monads, bacterioid bodies, and distinct bacteria. The zoospores are demonstrably derived from the mycelium, arising from common atmospheric spores; and there is every probability that the monads and bacteria have a similar origin. Thus, while Dr. Cunningham expresses no opinion on the question of the ultimate origin of bacteria, whether by heterogenesis or the contrary, his conclusions are to a certain extent at variance with those of Dr. Burdon Sanderson, who has been led to deny the transmission of bacteria or their germs through the air. As a rule, none of the higher forms of infusoria were present, a result as much at variance with the statements of Ehrenberg as were those of the observations on atmospheric dust.

A final series of observations was undertaken on the air of sewers, care being taken that the air should be really within and not merely in the neighbourhood of the mouths of the drains. Comparing the solid particles found in the air thus obtained with those of other localities, the principal points of difference were—

"The comparatively small quantity of the common coarser constituents of dust in the form of silica, &c.; the presence of oily matter in consider able quantity in several of the preparations; the existence of distinct bacteria in four out of the eight specimens; the constancy of the presence of greenish cellules, apparently the spores of aspergillus; and the prominent feature imparted by the relative abundance of fine molecular matter."

The presence of bacteria is very noticeable and

"Accords with Cohn's observations on their conveyance by watery vapour, and suggests that their apparent absence in ordinary atmospheric air is due, not to their not entering it in large quantities, but to the fact that, unless the amount of watery vapour present is very great, they lose their characteristic appearance by which, in default of movement, they can alone be recognised."

These observations again conflict with the exclusive reference of bacteria to contact-inoculation which Dr. Burdon Sanderson has deduced from his experiments.

Since the connection, if any, of such atmospheric germs with disease was the chief aim of the researches, statistics of the prevalence of five special diseases were kept during the period that the atmospheric observations were made. The diseases selected were diarrhœa, dysentery, cholera, ague, dengue, four of which were among the most prevalent forms of disease during the period of observation, though the fifth, cholera, could scarcely be said to be prevalent at the time. The statistics extend over not only the day of the microscopic observations,

but also three days before and three days after, and the results are given in elaborate diagrams. The only conclusion was that no connection whatever could be traced between the numbers of atmospheric cells and the prevalence of any of the observed diseases, there being no constant coincidence either of agreement or discord. Nor, again, was the presence of any special form of cell at all connected with the prevalence of these diseases.

These conclusions, though negative, must be considered as the most important reached by Dr. Cunningham's researches. They show that, whatever the physical contagium of epidemic diseases may be, it is not among the solid impurities yet detected in air, and supply a much-needed commentary on the facile generalizations respecting "Dust and Disease" which have been made popular at the Royal Institution.. The motto of Dr. Cunningham's work would surely be "Dust and no Disease."

On the other hand, very important "concomitant variations" are brought to light on comparing the amount of atmospheric dust with physical conditions. So that Dr. Cunningham is able absolutely to state—

"The amount of inorganic and amorphous particles and other débris suspended in the atmosphere is directly dependent on conditions of moisture and of velocity of wind."

We should add that the memoir is illustrated by fourteen well-executed plates, containing a vast number of figures, sufficiently minute to enable a botanist like Mr. Berkeley to recognise distinct organic forms, and that every page of the report is, to use Mr. Berkeley's words, worthy of careful study.

II.—Researches on the Agent or Agents producing Cholera.

The researches on cholera, conducted by Dr. Lewis and Dr. Cunningham jointly, must, of course, be regarded as the most important of all, as being, in fact, the pivot upon which the others turn. We have to notice two reports of these, one dated 1872, the other 1874. Each is divided into three heads, viz. (1) The microscopic examination of blood, (2) Experiments on the introduction of choleraic and other organic fluids into the system, and (3) Experiments on the section of the splanchnic and mesenteric nerves. It will be best to take together the observations under each head contained in both reports.

1. *Microscopic examination of blood.*—The observations under this head are of a very fundamental character, and do not refer to cholera alone. The necessity of this extension of the field of research is shown in the following sentence:

"In the instructions issued by the Army Sanitary Commissioners for the conduct of this inquiry particular stress is laid on the importance of accepting no statement bearing on the question of the mode of origin and diffusion of cholera as proven, no matter how distinguished the authority on which it may have been made, until an opportunity occurred for verifying it for ourselves."

The authors have frankly accepted the arduous task thus imposed, and begun with the study of the microscopic appearances of normal blood. Their methods of examination were two—first, to expose a small drop of blood on glass to the fumes of a two-per-cent. solution of osmic acid, and then mount the specimen in a solution of acetate of potash; secondly, to subject the blood to continuous observation in wax cells for periods varying from one day to nearly three months, the object of the latter method being to watch the changes which a small portion of blood undergoes when secluded from the external atmosphere, but in presence of an adequate amount of air and moisture. The advantage of these continuous observations may not at once be obvious to all readers, but they supply, in fact, a particular kind of information which cannot be obtained in any other way. It is desirable to ascertain not only whether any or what organic particles may be present in a specimen of blood, but of what, if of any, the germs may be present. By such observations it has been attempted to show the presence of sarcinæ in normal blood, and to establish the existence of peculiar corpuscles characteristic of syphilis, the phenomena being in both cases apparent only in blood which has been carefully preserved and subject to continuous observation during a certain time. The same method is adopted to show the development in some specimens of blood of bacteria not observed in it when fresh, and proceeding, in all probability, from some germs not visible or not recognizable with our optical resources. Moreover, the changes which the elements of the blood themselves undergo are subject to variation, and have been thought to be significant. The results arrived at in the first series of observations have lost some of their importance in consequence of the numerous researches which have been made of late years by other observers, but the comparative observations made on healthy blood and on blood from cholera patients are interesting and valuable. Cholera blood exhibited at first nothing abnormal. The number of white corpuscles at first visible was small and not noteworthy. Coagulation took place in the ordinary manner, a ring of clear serum being formed round the contracting clot;

"But with the formation of the ring of clear serum a series of most remarkable phenomena constantly presented itself. Normal-sized white corpuscles began to migrate into the fluid, but in addition

to these, and in far greater numbers and activity, were larger and more delicate bioplastic bodies; cells they were not, for they had not at this time the faintest differentiation of walls, contents, or nucleus. They were simply masses of fluid bioplasm, so fluid and diluted as to be scarcely distinguishable by refraction from the surrounding medium."

Gradually these masses became more distinct, and showed both changes of form and movements of progression. We cannot follow their fortunes minutely. Suffice it to say that, after subdividing and breaking up into a second generation of bioplasts, they became motionless, presenting the appearance of aggregations of pus-like cells, ultimately either breaking down into monad-like colonies or forming something more resembling nucleated cells. The active stage of the bioplasts, in sealed preparations, was not usually more than twenty-four hours, but in the motionless condition they sometimes remained unchanged for many days. The most important result of these observations on the blood is undoubtedly the explanation they are capable of affording of the bioplastic bodies and cells so abundant in, and characteristic of, the evacuations passed in cholera, to which the bioplasts of the blood bear the most marked resemblance, if they are not identical with them. The same is true of certain "hyaline-cells" found equally in the blood and in the evacuations, which appear to be modified bioplasts. It would seem, then, that the cellular elements found in the intestinal discharges are derived from the blood, and that in both situations they are modified elements of the body itself, not extraneous organisms. In fact, the blood of cholera patients was found, as an almost invariable rule, free from bacteria or other foreign organisms, either actual or potential—that is to say, they were neither present at the moment of examination nor seen to be evolved when the specimen was subjected to continuous observation.

These phenomena are not (and the authors draw attention to this point) wholly without precedent in the case of normal blood. In a certain number of normal specimens examined in the manner above described amœboid corpuscles, similar to those seen in cholera blood, were seen creeping out of the clot after coagulation, and went through similar metamorphoses. The difference consists in the far greater number of such corpuscles seen in the cholera specimens, and in their greater permanence, the bioplasts in normal-blood specimens rapidly becoming disintegrated. Still the authors believe that they could always distinguish a specimen of choleraic from one of normal blood on the second day of observation, even though they might appear indistinguishable when first examined with the microscope. This is certainly a solid result, if confirmed

by subsequent observation, and we must admit that the authors have discovered a *property*, if not a *criterion*, of the blood in cholera.

Other interesting observations, which we must pass over, were made on specimens of normal blood, especially in relation to the occurrence under ordinary conditions of monads or bacteria, which were found to be present only in a very few cases either of healthy or diseased blood, even when observed continuously for a considerable time. For these and other reasons, the authors express somewhat strongly the opinion that there is no evidence in favour of attributing any diseased conditions to the causative action of bacteria or similar organisms, even when they are proved to exist; but we hardly think they have always done justice to the evidence which has been furnished. For instance, to say that the bacteridia of Davaine, found in the blood in cases of splenic apoplexy of sheep, and malignant pustule in man, are nothing more than coagulated fibrine filaments, appears to us to be nothing more than a conjecture; again, the authors appear to us greatly to exaggerate the difficulty of distinguishing minute particles of an undetermined nature, which may have some mechanically produced movements, from bacteria, and it seems hardly worth while to quote such a writer as Béchamp. That this is difficult or sometimes impossible in blood preparations must be granted, because it is difficult to apply the crucial test of solubility in strong solution of potash. That bacteria, properly so called, are not dissolved by this reagent, while particles of albuminous and fatty nature are so, cannot admit of doubt; nor that collections of particles insoluble in potash are found in the solid tissues of patients dying of certain diseases. Neither does it appear to us at all decisive, or even very important, that such bodies should not be found in specimens of blood taken during life. Very few may be passing through the blood at the same time, and the distribution of these may be extremely irregular. The presence of such bodies in the tissues in certain diseases we regard as indisputable, though the question will still arise, what relation they bear to the disease, and Messrs. Lewis and Cunningham may be quite right in supposing that the explanation of cholera is not to be found in this direction.

Again, we quite adopt the explanation indicated by the authors in their second report on cholera (p. 32, 1875), that the occurrence of bacteria of ordinary putrefaction may be the expression of the fact that changes usually observed after the death of the organism have taken place in certain diseased fluids and tissues before death. Such cases, *e. g.*, gangrene, urinous infiltration, &c., are, we think, distinguishable without difficulty

from cases of pyæmia or certain specific diseases where bacteria not resembling those of ordinary putrefaction occur in the blood during life or the tissues after death. One suggestive observation is recorded in the second report, viz. that leucocytosis, or an abundance of white corpuscles in the blood, appears to be inconsistent with the coexistence or development of bacteria. The latter, if introduced into the circulation, disappear, being apparently worsted in the struggle for existence, so long as the leucocytes are in a state of activity. In vaccinia produced by inoculation and in syphilis the authors found bacteria to be as little constantly present as in cholera.

2. *Introduction of choleraic and other fluids into the system.*—The experiments on the introduction of choleraic and other fluids into the system form two important series. In both series dogs were the animals chiefly experimented upon, at least in those experiments which are recorded, it being found that great fallacies attend the performance of such experiments upon smaller and more delicate animals, such as the rodents.

In the first series seventy-nine such experiments are recorded, in which the matter of choleraic dejections, or a solution of it, or in some instances other organic fluids, such as blood, either fresh or kept for some days, were injected into the veins of dogs, being also in some experiments introduced into the peritoneal cavity. Anæsthetics were used in the most careful and complete manner, so as to spare all pain to the animals, and besides thorough post-mortem examinations the blood was in most instances examined with the microscope.

The general results of this first series of experiments were as follows:—The injection of choleraic material into the veins proved fatal in about forty per cent. of the cases, and excluding cases where death appeared to be due to the operation the rest of the cases recovered. The mortality was less when the material was much diluted, but it was greater when the material had been kept some days than when it was fresh. The injection of ordinary fæcal matter was fatal in a certain number of cases, the average mortality being about thirty per cent. instead of forty, a difference which does not seem clearly to show any specific lethal property in the choleraic matter. The chief peculiarity noticed at the post-mortem examination was that great disorganization, or at least congestion with other complications, affected the whole of the *small intestines with the exception of a portion extending one or two feet above the ileocæcal valve.* The authors cannot in any way account for this special distribution of morbid change or reconcile it with any known facts of anatomical structure. No similar limitation is seen in cholera. No special affection of the intestinal glands

was in any instance observed, and the stomach and large intestine were always healthy.

In three cases large numbers of vibriones or actively moving filaments were observed on the mucous surface of the intestines and detected further in the mesenteric glands, but not in the blood—a condition which the authors compare to what is called *mycosis intestinalis;* but with this exception no relation to vibriones or bacteria was made out. It is a little singular that the same materials injected into the peritoneum produced a somewhat different series of morbid changes. Peritonitis of course resulted, and in a considerable number of cases pericarditis; but in the intestines no disorganization of the mucous membrane was observed, great congestion and a sort of sanguineous exudation being alone remarked.

This series of experiments, then, failed to induce lesions or phenomena identical in nature with those of cholera, nor did "it afford any evidence in favour of the existence of a specific poison contained in choleraic excreta peculiar to them alone, and giving rise to special phenomena." It clearly points, however, to the influence of decomposing animal matter, even when not extremely fœtid, in producing intestinal disease.

The number of these experiments did not appear sufficient to warrant any inference as to any difference in *degree* between the toxic effect of choleraic and other alvine discharges, nor did it appear to the authors that a sufficient number of experiments had been made with perfectly fresh choleraic material. To supply these differences was partly the object of the second series of experiments, but they were also intended "to test to the utmost the influence of bacteria in these processes," and, lastly, "to ascertain whether the product resulting from lesions thus produced invariably possesses the property of reproducing the phenomena in a more marked or even equal degree."

The general result of the two series of experiments was that, while both choleraic dejections and those from persons in health may when injected into the veins produce intestinal affections, this result was produced much more frequently by the choleraic material. Fresh cholera material appeared to be undoubtedly more poisonous than that which had become putrid; and what is a very important fact, heating the material to boiling did not in the slightest degree diminish its poisonous effect. Almost the same was true of the toxic properties of ordinary alvine evacuations, so that the authors draw the unhesitating conclusion that the cholera poison cannot be a living substance, no such substance, they think, having been proved capable of surviving exposure to a temperature of 212° in fluid.

It is noteworthy that the authors could not confirm in any

way the fact recorded by so many experimenters with respect to artificial septicæmia, namely, the increase in intensity of the virulent properties of inflammatory products caused by their transference from one animal to another. In fact, solutions of excrementitious matters introduced directly into the peritoneum were more poisonous than the fluids resulting from the inflammation thus caused—a result at variance with those of Burdon Sanderson and Klein, and of the French experimenters. In only two instances were they able to transfer the morbid action more than twice. The only conclusion we can draw from this fact is that it suggests a multiplicity of poisons, some, perhaps, being more capable of rapid increase than others.

It is fair to give the final conclusion in the authors' own words:

"Something is present [in choleraic matter] which is capable of exercising a singularly pernicious effect on animal life, the most prominent local manifestation of its action being observed in the intestinal canal.

"What is this something? Is it visible? Is it a living substance?

"With regard to the first question, we should not presume to speak decisively, though we have ourselves searched for it in vain with lenses which have the reputation of being the best yet constructed, and have been uniformly unsuccessful in associating it with any constant visible phenomena.

With regard to the second question, the authors think that—

"Until it be proved that living substances can withstand immersion in a fluid at a temperature of 212° F. of some minutes' duration, we have no hesitation in stating that the morbid phenomena which we have observed to follow the introduction into the animal economy of strained solutions of choleraic and normal alvine discharges, and of other decomposing animal substances, are not the result of infection with a material the poisonous properties of which are dependent on its possessing vitality."

3. *Section of intestinal nerves.*—Before leaving the subject of cholera, we must refer to the experiments on the section of the nerves distributed to the intestines, first performed by Moreau. This experimenter found that, when the branch of the mesenteric nerve distributed to a loop of intestine was divided, the paralysis thus produced was accompanied by a copious secretion of watery fluid from the mucous membrane. Now, comparing the results of the experiments just detailed, in which choleraic or other animal matters were introduced into the circulation, with the phenomena of actual cholera, one of the most striking differences is the absence, in experimental conditions, of that increased secretion of fluid which is so characteristic of the disease. The authors therefore hoped that, if

they could in any way induce a greatly increased secretion, they might assimilate the morbid phenomena of experiment somewhat more closely to those of disease. Hence they determined to repeat Moreau's experiments, but first attempted to ascertain whether by section of the splanchnic nerve the whole of the intestine simultaneously could be affected, and not merely one or two loops as in the method of the French experimenter. The result of two series of experiments was, however, to show that section of the greater splanchnic nerve on one or both sides, even when combined with excision of the semilunar ganglion, had no effect whatever on causing exudation or other change on the intestine. By section of the mesenteric nerves, on the other hand, they confirmed in a great number of instances the results of Moreau, the contrary instances being apparently due to imperfection in the experiment. The almost invariable result was a copious secretion of fluid, entirely independent of the detachment of the epithelial covering, but sometimes associated with an exudation of bioplasts upon the surface. Similar confirmation has, we may say, been given to Moreau's statements by Dr. Lauder Brunton, and the subject is now under investigation by a committee of the British Association.

Let us hope that Drs. Lewis and Cunningham, having already reproduced, in some degree, two of the chief morbid phenomena of cholera, may be ultimately able to effect the synthesis of the whole, and thus explain, in the only really satisfactory way, the whole morbid process.

IV.—Researches on Entozoa.

Dr. Lewis's investigation of the disease known as chyluria has led to very interesting results, some of which may be now pretty widely known; but as others have scarcely received due attention, it will be worth while to give a brief *résumé* of the whole subject. In the year 1870 Dr. Lewis first observed in the milky urine of a patient suffering from "chyluria," numerous specimens of a small nematoid worm, which, being constantly found and evidently not accidentally present, was referred to as the probable cause of the disease. Some fifteen or twenty cases subsequently examined fully confirmed the hypothesis, filariæ being present on every occasion. At this time the parasite was not detected in the blood, but about two years afterwards, when examining the blood of a patient with diarrhœa, Dr. Lewis observed nine minute nematoid worms on a single slide. The investigations to which this incident gave rise have now satisfactorily established the presence of the parasite in the blood as well as in the urine in all such cases; and the name *Filaria sanguinis hominis* given to the parasite

has been shown to be well deserved. So numerous are the parasites that they may always be seen in a minute drop of blood taken from any part of the body; and supposing them to be uniformly distributed, Dr. Lewis calculates that there may be, on a moderate estimate, 140,000 in the body at once. In one case the patient affected with chyluria suffered from a chronic "inflammation" of the conjunctivæ, and milky fluid was constantly welling from the corners of the eyes, which, on examination, was found also to contain the parasite. Although the body is so universally affected, nevertheless no characteristic post-mortem appearances were recorded in the few cases where an autopsy was obtained.

"Not the remotest clue is afforded as to the nature or as to the cause of the disease by the *post-mortem* appearances visible to the naked eye; nor is there any sufficiently-marked lesion to account for the condition of the urine during life, nor for the rapid manner in which the patient (in one case) ultimately succumbed."

The microscopic examination of the kidneys, however, showed that numerous minute filariæ were contained in every part of them.

The constancy of these results was of itself quite sufficient to establish the fact of the association of the parasite with the disease, its habitat in the blood, and its elimination by the kidneys. It still appeared strange that the constant infection of the blood should have been overlooked in the earlier instances where the chylous urine was found to contain parasites, and a remarkable coincidence enabled Dr. Lewis to supply this omission. It happened that a compositor in the Government printing-office, who was actually engaged in setting up this very essay, was the identical patient in whose urine two years before filariæ were first detected, and whom Dr. Lewis had vainly endeavoured to find for the purpose of further examination. No difficulty was found on this occasion in obtaining numerous filariæ from a few drops of his blood. The man had been engaged in his occupation without suffering inconvenience from his complaint, and indeed in many instances the disease appears to have been extremely slight, so that the patients refused to remain in hospital to be treated, or even concealed their condition. The incident illustrates also the length of time (two years and a half) for which filariæ can remain in the body without giving any evidence of a tendency to undergo further development. In fact, as Dr. Lewis says—

"For aught we know to the contrary, these filariæ may live for many years, and thus at any moment, no matter how long after a previous attack, nor in what country the person may reside, he may be

surprised by the sudden accession of chyluria, or any other obscure disease, such as will be readily understood by the physician when he becomes aware of the state of the blood."

We cannot dwell upon the anatomy or the life-history of the hæmatozoon. It is a worm-like animal, the average diameter of the body being about that of a red blood-corpuscle, and its average length about forty-six times its greatest diameter; that is to say, its greatest transverse diameter is about $\frac{1}{3500}$ of an inch, and its length about $\frac{1}{75}$ of an inch. When seen on the stage of the microscope it is extremely active in its movements and difficult to observe, but the body is evidently contained in a transparent, elastic, tubular sheath, closed at both ends, which is capable of elongation or shortening. This seems to show, Dr. Lewis thinks, that in the present stage of its existence the home of the parasite is in the blood, as it has no visible means of perforating the tissues, and does not show any tendency to migration. There is evidence, however, that it may also penetrate the minute channels of the lymphatic system. From what we know of the life-history of parasites it is clear that that of the hæmatozoon, thus far known, is quite incomplete. This is an embryonic form which must have other stages and transformations, though these are at present quite unknown. Dr. Lewis conjectures merely that the parasite will ultimately be traced to the tank, either to its water or its fish.

In the second report on this subject another formidable disease is referred, with great probability, to the action of the same parasite, viz. an elephantoid condition of the scrotum, and also in one instance of the foot. We do not gather that Dr. Lewis attributes all the monstrous tumours known as elephantiasis to this cause, but in the cases recorded there was infiltration of subcutaneous tissue with fluid, and hypertrophy of the surrounding parts, so as to produce a large tumour, which in one case was of the size of a man's head.

Although, as we have said, the life-history of the *Filaria sanguinis hominis* is quite incomplete, and even its transmission through the body imperfectly understood, much fuller information has been obtained on both these points with respect to an allied form of nematode hæmatozoon from the dog, which forms the chief subject of Dr. Lewis's second memoir. The parasite there described appears to be quite distinct from the blood-parasites which have already been found in the dog by different observers, and enumerated by Dr. Lewis in an historical sketch, which we have not space to notice. It has been found in two stages—the embryonic and the mature. The embryo bears a close resemblance to the human hæmatozoon spoken of above, and, like that, lives in the blood. The mature form (or

what is believed to be so) is a nematode worm from one to three inches in length, corresponding very nearly, though not precisely, to the *Filaria sanguinolenta* of Rudolphi; it is found in solid fibrous tumours along the aorta and œsophagus. Dr. Lewis's belief is that the mature worm is produced from an immature larval form, which is swallowed by the dog, and attaches itself to the mucous lining of the œsophagus, then working its way through the tissues till it reaches the thoracic aorta where the worm lives coiled up in a small nodular elevation on the inner surface of the vessel. Here it enlarges; ultimately adjacent worms migrate into each other's abodes, and a tumour the size of a filbert or larger may be formed, which, when cut into, contains a little colony of mature pinkish worms, the male being from one to two inches long, the female from two to three and a half. The females become impregnated and produce innumerable ova, which are imbedded in a thick, yellow, gelatinous fluid, and pass through an orifice in the tumour, either into the blood on the aortic side or into the alimentary canal on the œsophageal. The ova develop into embryonic worms, which are the hæmatozoa already described, and these appear to undergo no further transformation in the blood. Of this history there is, as Dr. Lewis confesses, no direct proof, but we must allow it a very high degree of probability. The human hæmatozoon (though a distinct species) must almost certainly have a similar history, and hence, to understand its development, we should have to find a filaria somewhere in the solid tissues of the body. We may only point out that the *hæmatozoic* stage can hardly be a normal stage in the metamorphosis of either species. The embryos which find their way into the intestinal canal and escape must be those which propagate the canine species, though in the *Filaria sanguinis hominis* the immature form ejected in urine or lymphoid discharge may undergo further transformations in some appropriate field. Those that remain in the blood stop at the same immature stage of development.

The whole of the two reports on hæmatozoa, which we have been obliged to notice very imperfectly, will well repay perusal,[1] and are, like the others, admirably illustrated.

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[1] An abstract of the second report, with figures, will be found in the 'Quarterly Journal of Microscopical Science' for July, 1875.

VIII.—On Inter-Marriage.[1]

MARRIAGES of kinsfolk must have been coeval with creation. Whether the race originated in one pair, or in pairs corresponding in number to its varieties, such unions in primeval times were inevitable. Moreover, wherever communities or colonies were founded or formed by a few individuals, or wherever such associations were or are isolated geographically, or by race or social conditions, the same course must have been, and must still be, pursued; or where the proportion between the two sexes is destroyed by the infanticide of female infants or otherwise. It would be premature to inquire whether such arrangements were beneficent or pregnant with evil, as, under the circumstances, their consequences could neither be known, nor dreaded, nor escaped from. But, independently of the limitation of choice, other influences may have contributed to multiply such alliances. It can be readily conceived that early associations, the affections, the friendship subsisting between relations, the strong sympathies created by an identity of language and interests, by a similarity in habits, pursuits, predilections, may have brought about, under the agency of freewill and preference, by the very attractions between the sexes which have guided men in all ages, those consanguine connections which, under certain circumstances, seem to have been more the result of the tyranny of position than of the promptings of instinct, for that dissociation or divarication between members of families, now so glaring, must have occurred in comparatively modern times. In the daylight, as well as in the dawn, of civilisation, even now, there are families who consciously and advisedly shrink from the formation of new connections, marry within the bounds of consanguinity, and have hence been called cousin-families. Even in our own country, amongst the remaining representatives of the Celtic race, within a short period, and in other countries at the present time, clanship has been maintained by cousinship, and the pride of pure blood was as indomitable in the vassal as in his liege lord—in the serf, who could scarcely trace his genealogy, as in his chief.

The Bible, taken strictly as a biographical record, and under this aspect it has never been assailed by sceptics, reveals the

[1] 1. *The Marriage of Near Kin, considered with respect to the Laws of Nations, the Results of Experience and the Teachings of Biology.* By HENRY HUTH. London, 1875. Pp. 426.

2. *Marriages between Cousins in England and their Effects.* By GEORGE H. DARWIN, M.A., Fellow of Trinity College, Cambridge. 1875. Pp. 29.

3. *Medical Problems of the Day. The Annual Discourse before the Massachusetts Medical Society,* June 3, 1874. By NATHAN ALLEN, M.D., LL.D. Lowell, Mass. Boston, 1874. Pp. 92.

course which must have been long followed by primitive peoples. Brothers must have formed unions with sisters, and so on, even if nearer ties were not occasionally created; and even in what may be called the mediæval period of Jewish history, when the exigencies of a sparse population had disappeared, we find that even leaders and lawgivers sanctioned connections between blood relations; and even after legal restrictions upon such had been imposed there remained a national prejudice against marriage with aliens. Wherever the genesis of human association has to be recommenced, or wherever small communities are shut out from surrounding peoples by accident, as in the case of the mutineers of the Bounty,—by design, as in that of the Vaquéros of the Pyrenees, and the voluntary recluses of St. Kilda —or by compulsion, as in isolated tribes in Iceland, Westmannoë, &c. &c.—similar institutions have prevailed. Motives of a higher or lower character, as they may be variously estimated, have induced and justified the same relation between the sexes. The Ptolemy rulers of Egypt, either in obedience to an ancient custom or to guard against the pollution of the royal blood and caste by admixture with subject or servile, and therefore impure, streams, habitually married sisters, nieces, and cousins. This incestuous intercourse prevailed during thirteen reigns at least. Impelled by similar incentives, the Peruvians and Persians are proved—and the inhabitants of a large portion of the world, designated by the Greeks, barbarian, including India, Mesopotamia, Parthia, Assyria, all the north of Africa, are asserted—upon good authority, to have chosen their mothers, daughters, or sisters, as wives. In fact, it would be difficult to point out any region in which, anciently, this custom did not prevail, especially among the dominant and privileged classes; and even our own ancestors, while in a semi-savage condition, imitated their more civilised contemporaries. But whole tribes of men, not actuated by the desire to transmit an uninterrupted and uncontaminated lineage of princes and rulers, but by the wider conservative principle of keeping their people free from all foreign ties and influences, have encouraged marriage between members of the same sept or tribe. The Arabs, before their conversion to Mohammedanism, practised incest in its grossest form, and the Bedouins are still so proud of their race that they will not marry an alien, or even a female of different descent, and are as rigid in carrying out this rule as they are in preserving pure the pedigree of their horses. The Affghans and natives of Beloochistan, all presenting a magnificent physical development, are so proud of their lineage, and so exclusive in guarding it, that recognition of incorporation is never obtained until six descents can be proved, and they, accordingly,

never ally themselves with even neighbouring, and, perhaps, congeneric clans. The Parsees, or Guebres, are as scrupulous and exacting as to genealogy, although religious as well as tribal scruples may regulate their conduct. The Gypsies of the same region live almost promiscuously, the children belonging to the horde, and not to particular parents, as is affirmed to have been the rule formerly in China, where even mothers remained members of their sons' harem; and the Mingrelese on the Black Sea, handsome and beautiful in person, associate nearly in the same degraded manner, but whether from selfish, territorial, or patriotic motives has not been ascertained. But in Hindostan there is an *imperium in imperio*—a separation of a vast population, which must have had a common birth, into sects and castes; first, into four great divisions, which are again differentiated into more than a hundred others, by descent, by occupation, and by moral antipathies. They will not intermarry, they will not help, tolerate, touch, each other, so that Brahmins are united to Brahmins, and Sudras are united to Sudras, through successive generations, and, unless where the written or traditional custom is violated, and the distinctions of society outraged, the connections between blood relations in given localities must be intimate and intricate. The Ceylonese acknowledge the same tyrannous prohibition against the marriage of the high and the low, and the rich and the poor, and incest, polygamy, and polyandry are the natural concomitant, if not direct results. The latter revolting tie exists in Thibet as it anciently did in Sparta, as well as in Central Africa, but under very different domestic and social regulations, and upon what pretext is not well ascertained. Where polygamy is allowed or enjoined by law, as among the Mongols, a man may marry three sisters at once, and two families may intermarry for centuries. Creed, superstition, sectarianism, even politics, which lead so largely to the segregation of communities, are as fertile in bringing together in closer and nearer affinities the individuals composing the sections separated. Marriage within the pale was, at one time, a cardinal virtue and an omnipotent rule. Even in the very remote and pleasant land of Polynesia the royal race contracted marriages with blood relations, and "Tamehandra married a relation, while his son married a sister, and, from love to his father, one of the latter's widows." The marital affinities in these islands, where all save the human soul is beautiful and divine, are of the most lax and indefinite character, and are determined rather by passion, temperament, or abundance of food, than by any of the factors previously enumerated. It should be here noted that the Circassians, consisting of several fraternities, never marry within their

limits, that the Kalmucks have a great abhorrence of marriages between near kin, that the Tartars do not seek wives within their own clan, that the Dyaks, among whom the decapitated head of a foe is the most acceptable gift to an affiancée or bride, are interdicted from marrying first cousins, in order to place prominently the proposition that such repugnance must depend upon individual, or national, or climatic differences and mutual constitution, as the majority of mankind have at one stage of progress or another followed an opposite course.

At certain periods of advancement almost all nations have instituted prohibitions against consanguine unions. The reasons for such a step, the grades of relationship embraced in the law, and the extent and modification of the restrictions, differ widely in different races. Sometimes the interdict is connected with the inheritance of property, sometimes with asceticism, but rarely with any perception of the laws of health or with the evils supposed to arise from the violation of these. In fact, it is most probable that the belief in the existence of deterioration has been suggested by such enactments and by the rigidity with which, both in heathen and Christian lands, they were enforced. Had degeneracy been palpable and clamant, it must have appealed so constantly and clamorously to the hearts and consciences of families and sodalities that no police, or penal, or priestly interference would have been required, and it may be argued that, had the infirmities of body and mind now traced to this source been multiplied in proportion to the frequency of the practice, the blood-corruption must have been so universal and so appalling that it could not have failed to attract the notice and excite the fears of the most ignorant and stolid of the governed as well as of the governing body. To the Israelites a code of regulations as to marriage was promulgated between 1571 and 1620 B.C., when the circumstances determining the earlier post-diluvial marriages had ceased to press. By this code, tantamount to the condemnation of previous connections, a Jew was not permitted to marry his mother, sister, step-mother, and other degrees within blood-kinship. A slight amount of restraint was created in Egypt by considerations of rank, riches, and the sacredness of caste; but there was, besides, no union permitted in the direct ascending line, nor collaterally with a sister. A check is said to have been given by Alexander the Great to the incestuous intercourse between members of sovereign Persian families. The Greeks were, like other nations, surrounded by barbarians, endogamous, and their proliferation by emigrant colonies was the result of the exclusion of children of mixed descent from citizenship, and it is presumable that the disposition to breed in and in was not

effectually counteracted until the effect of Christianity claimed the supremacy over the selfish impulses, although the same limitations prevailed as in Egypt. Rome, in its rise as well as in its decline, seems to have equalled less powerful and less polished communities in the contraction of blood-marriages, although perhaps the most flagrant and frequent instances are to be found in its lusty youth. Such ties seem to have been dictated by pride, patricianism, and the transmission of property. The Tarquins were but examples of the practice among the nobles; all ranks recoiled from espousal with a foreigner; the equestrian disdained a plebeian connection; heiresses were bound to marry kinsmen. The union of uncle and niece was permitted. Mohammed originated wide and strict prohibitions, but he himself married his first cousin and a daughter-in-law; but Constantine and Constantius attached the penalty of death to such marriages, and Theodosius introduced a law against the marriage of cousins. Many fluctuations of opinion and legislation may be detected, even after Italy and other parts of the Western Empire had been converted to Christianity, and no fixity or permanency was imparted to the extent of prohibition until the time of Gregory I, A.D. 550, who denounced the union of third cousins, although he had been anticipated by the Ostrogoths, who interdicted marriage within the fifth and sixth degrees, while Gregory III, A.D. 731, extended this restriction to the sixth or seventh degree, or between sixth cousins. It would be profitless to prosecute further research during the Christian era, as the canonical laws above alluded to have been observed with greater or less fidelity ever since among the lower orders, over whom the priesthood exercised, and still exercises, great, if not supreme, authority. But among the royal and princely families of Europe the dispensing power of the Church has so broken down these bonds and barriers that even heralds have a difficulty in unravelling the tangled web of propinquity produced; and had vengeance, under the guise of morbidity and mortality, really pursued the offenders against a sanitary as well as a sacred law, no representative of imperial or feudal lords could now exist. Nor has the shadow of such ecclesiastical absolution from the impediments of canonical law altogether passed away. The modern histories of Spain and Portugal afford many illustrations of this, and thus in 1826 a Papal bull would have set aside the prohibition of marriage between an uncle and a niece in the case of Donna Maria da Gloria and Don Miguel, but was nullified by the treachery of the latter; and even the plebeian mountain shepherds called Vaqueros, who will not marry out of their tribe, still pay more money for dispensations than the whole province of the Asturias.

Royal edicts have in like manner set the canonical laws as to consanguinity at defiance, and within a short period such a relaxation was passed in Sweden permitting the marriage of first cousins, and in the same country a man may marry his sister-in-law by the king's authority. It may be inferred from what has been stated that there was no inherent dislike or indisposition on the part of relatives to marriage; that, on the contrary, there existed many inducements to such connections, and that, although a different doctrine has been propounded, to the effect that an identity of sentiment and impulse, even in widely separated peoples, may have led to the establishment of similar customs, the only natural impediment is in the age of the parties. It is well known that, whatever the views of the early Jewish Lawgiver, whether dictated by inspiration or religious policy, these have, for better or worse, guided civilised nations since the epoch when they were delivered. Gregory is said to have reproduced them, and the Church of which he was a Saint is conceived to have injured the world by placing limits upon the increase of population, while it marvellously raised the standard and sway of morality by placing woman in a new and purer and more dignified relation to the other sex. The conventual life, the withdrawal of large numbers by celibacy from the reproductive classes, must have tended to disturb the equilibrium between the sexes as to marriage, and must have served as an indirect check upon fertility. It has been affirmed that the cloister was resorted to as a sanctuary from unholy unions and from the curse which was conceived to impend over the fruit of those who eat of the forbidden tree. What, then, was this curse? Did it bear the fruit of mere moral and ultimate retribution—was it wreaked in physical disease, deformity, decrepitude, in sterility, impaired fecundity, idiocy, paralysis, and all the ills that flesh is heir to, culminating in early or premature death? In contemplating this sad picture of the future of humanity we have often been struck by the habit of intelligent and laborious advocates of the hypothesis of the pernicious and fatal effects of consanguine marriages in overlooking the cosmic custom which seems to be an intuitive refutation of such hypothesis; in nibbling over the two or one and a half per cent. which may distinguish the condition of the children of those conjectured to be unhealthy from those of parents conjectured to be healthy. It is at the same time astounding that these inquirers should concentrate their attention upon consanguinity alone as the origin of physical and psychical degeneracy, even of certain forms of moral turpitude in small communities or circumscribed localities, to the exclusion of all the other known factors of such afflictions which must be contemporaneously at work, and which are confessedly productive

of similar results, and this while the more candid of the number admit that the data obtained are defective and often selected; that such data "do not speak strongly against a blood-relationship of parents" that this influence, whatever it may be, is "unsteady," and that they have been disappointed as to its uniformity, extent, &c. Two methods have been followed in the attempt to answer the vexed question as to the effects of blood-relationship on offspring. The first of them has consisted in selecting an isolated village or tribe, in ascertaining the number of marriages, sanguine and non-sanguine, among the inhabitants, the number of children in these unions respectively, then in comparing the health, strength, survivorship of these, and in concluding, from the proportionate value of these elements, the influence of relationship, but, as we have just said, without any due consideration or estimate of the other causes, such as heredity, race, food, habits, moral and religious training, which must affect this proportion. The second method consists in taking similar steps and in making similar deductions without making due allowance for the circumstances which may prevent, or modify, or destroy, whatever occult influence may be associated with close and long-continued intermarriage.

The objects of the work of Huth now before us are professed to be "to put in a collected form all that has been written on the marriage of near kin," and "that it may serve as a handbook to the literature of the subject." These expressions are too modest, and are calculated to mislead as to the amount of copious, patient, and exhaustive research displayed, as to the clear and, we conceive, candid exposition of a controversy of great width and vast importance, and as near an approach to a settlement of the dispute as can at present be expected.

The first portion of the volume is dedicated to a sweeping survey of marital usages in almost all lands and ages, and demonstrates that, however irreconcilable the conjugal connections narrated were with current opinions, they could not have entailed disastrous consequences; that, even where the kindred of the parties was close and long continued, the issue was robust and beautiful, as exemplified in the Lacedemonian women; and that the descendants of the Jews, even at present, are in favorable surroundings, equal in many respects to the Circassians, who are rapidly increasing in numbers, and present a smaller infant mortality by one third or one half than among the Christians. The same remark applies to periods when females were bought, sold, and treated, though with less solicitude and science, like our cattle.

The following portion on asceticism shows that, although ecclesiastical laws may frustrate or limit, fanaticism may foster,

promiscuous and profligate intermarriage, and that sects at present exist in which incest is an institution, equally as in the days of the Magians when such corruptions are said to have arisen as to countenance the axiom that the offspring of a son and a mother was the best calculated for the office of a priest. The succeeding chapters are on the varied origin of prohibited degrees among savages, and on the absence of all instinctive antipathy to consanguine union, wherein it is stated that Burton, in his 'Anatomy of Melancholy,' is the first author who "considers that the offspring are hurt by these marriages;" and when the weight of evidence gathered from a large catena of recorded observations is in their favour we reach the crucial argument in the investigation derived from isolated communities who have constantly married among themselves. Among such are enumerated the descendants of the mutineers of the "Bounty," who were finely formed, strong, and of tall stature. A tribe at Surabaya, Java, 1200 in number, occupying forty villages, are so proud of their institutions as never to admit female strangers within their pale, and are frugal, happy, bigger and stronger than any neighbouring race. The same amount of vigorous life and of longevity has been noticed among the natives of villages in Cornwall, where exogamous marriages are, from situation, difficult; among the Stuarts of Glenfinlas, where clanship determined marriage. In Burnmouth and Ross, Berwickshire, where position and contraband trade long confined matrimony within narrow limits, though it be affirmed that there is a larger amount of "unsoundness" than in the average population of Scotland, the inhabitants are said to be healthy, strong, industrious, and respectable; as in Eyemouth, in the same county, which has a similar site in history; in St. Kilda, where a small group of individuals is gradually verging towards extinction, partly from insufficient or inappropriate nourishment, but chiefly from *trismus nascentium*, where unions within the interdicted degrees have been inevitable for centuries. Iceland, where a population of about 58,000, practically shut out from other parts of Europe, is and has been compelled to marry in and in, although sometimes decimated by epidemics and subjected to frightful hardships, presents a greater number of births, less deformity, less insanity, than in Denmark; although the percentage of idiocy is greater, it is wonderful, when the somewhat dissolute manners, the intemperance, and the extreme destitution of the majority of the parents are considered, that it is so small. In the Scilly Islands the inhabitants are described by one narrator as dwarfish, by another as possessing proportions which "give the lie to the current notion that men and quadrupeds must degenerate in small islands," and here the boundaries

drawn by degrees of affinity must be constantly overstepped. Passing the channel, we find in Asprières a population of 1700, with only two deaf-mutes springing from unrelated parents. In Baty, according to Voisin, there is a population of 3300, separated from and having little communication with the mainland, simple, intelligent, reserved in character, presenting no instance of malformation, or any disease of the mind, or of deaf-mutism, albinoism, blindness, or *retinitis pigmentosa*, and free from the stains of prostitution, drunkenness, and other immorality, although marrying amongst each other from time immemorial. In the nominal republic of Andorra, Pyrenees, the citizens, amounting to 7000 or 8000, living as their fathers lived in great simplicity, never unite themselves to foreigners, nor to any one unequal in rank, and consequently choose their helpmates from their own family and such as participate in their fortunes. This unchanged and unchanging people are described as a strong and well-proportioned race, among whom mental disease and vice are almost unknown.

We have intentionally omitted a large number of interesting and pertinent illustrations from this catalogue, most of which militate against the notion that marriages of kin entail evil, some support it, and lastly that derived from the present condition of the Jewish race, especially in relation to deaf-mutism, because the information on the subject is conflicting. This has been done, not merely to economise space, but because we object to skeletonic and microscopic statistics, and, in the words of Huth, because "we have absolutely no basis or very imperfect estimates indeed" in the face of world-long history and experience with which the great mass of mankind rest perfectly satisfied. Another series of facts can only be very cursorily adverted to; such are the connection between idiocy, microcephalism, deformed and dwarfed stature, with the soil and surroundings, as seen in the Cretins of Alpine regions; the direct or immediate connection between idiocy and alienation with intemperate habits, as proved by the increase of idiocy 150 per cent., and insanity 50 per cent., on the removal of the spirit duty in Norway; the fluctuations of intemperance from social habits, and in particular classes, independently altogether of family relations; the absence of consumption from communities where frequent consanguine marriages are unavoidable, as in Iceland and in our own Western Islands, from which evidence has been drawn to prove that idiocy, &c., can be traced to this source;[1] the arguments of Chateauneuf, who concludes that "seven or eight consecutive generations, lasting about three centuries, is the average life of a nobleman's family in France and that, so far

[1] Dr. Mitchell 'On Blood-Relationship in Marriage,' p. 442 *et seq.*

from being less prolific, they are perhaps even more prolific than ordinary families;" the etiology of scrofula from every enfeebling and exhausting origin in no way connected with consanguinity; the theories that anæmia in the parents may explain all the phenomena under discussion, and that crosses are positively, as well as potentially, injurious by spreading and multiplying defects in the type, as seen in the deterioration of mixed races and half-castes, which unite the vices and defects of both parents.

The experience afforded by the breeding of the lower animals is valuable. The great preponderance of opinion of those who, experimentally or for gain, have engaged in this inquiry is that consanguine connections afford a direct means of improving the breed, either as to a part or the whole of the body. This improvement consists, not merely in increasing the quantity or quality of fat, flesh, wool—saleable commodities which are, however, the products of diseased action in the animal—but of beauty of form, accelerated speed, and high development of some of the external senses. In fact, the great cardinal point seems to be that, provided the parents are healthy, their progeny will be so, will represent the desirable features of the type, and that these, under judicious management, will be intensified and perpetrated by long-continued breeding in and in. The racehorse of England, the Arab horses of the desert, can all be traced back to the same famous stock in one or more lines, by repeated and the closest interconnections, with the qualities for which they are prized either perfectly preserved or enhanced. In the thirteenth century sheep were imported from England by the Spaniards, who have excluded all foreign blood from the stock, which has been bred in and in since that time, and the animals are still free from all blemish or impairment of those qualities for which they have been prized. In France the Naz Flock has had no addition from without for sixty years, and gives no indication of degeneracy. The best breed now scattered over all the states of the American Union is descended from one Ewe, and have preserved her characteristics by constant in-breeding and without admixture. The wild as well as the park deer in Great Britain are free from all blight and blemish, although crossing is not within their reach; but the innumerable troops of wild cattle and horses which now cover the plains in South America, the Falkland Islands, &c., which must have proceeded from common parents, and could have had no access to other flocks, afford extraordinary instances of undiminished prolificness and of the possession of some of the most highly valued qualities of their species. But a wider survey of all the living creatures peopling the globe, and which must have multiplied at certain

times, and must still in many localities multiply, by incessant interunion, teaches the same lessons and more impressively. That evil results may follow the mingling of kindred blood cannot be denied, but that in drawing deductions from such cases the collateral influences should be embraced is well shown by the experiment of M. Legrain, who treated two series of rabbits, originating in a common stock, in a different manner, brothers being coupled with sisters during five generations. One series was kept in the dark, with unwholesome surroundings; one in light, with abundance of air, food, &c. The former, in the third generation, had diminished in prolificness and viability, while the latter, in the fifth generation, remained prolific and undegenerate in health.

We shall not analyse the concluding chapters on the question, "Why are there two sexes?"—hermaphroditism, &c.,—but would strongly recommend the perusal of the appendices as containing, in addition to the references in the body of the volume, a large collection of condensed cases bearing upon the matters discussed, drawn from a multitude of sources, and which present a sort of vidimus of the literature bearing upon consanguine marriages.

On opening Mr. Darwin's pamphlet on 'Marriages between First Cousins in England, and their Effects,' we were at first impressed with the suspicion that it was a humorous exercitation or philosophical conundrum, as in his third paragraph he writes, "In looking through the marriages announced in the 'Pall Mall Gazette' I noticed one between persons of the same surname. Now, as the number of surnames in England is very large, it occurred to me that the number of such marriages would afford a clue to the number of first-cousin marriages;" but, on discovering that this method, which many will stigmatise as novel and rude, was designed to supply that information sought for by the motion of Sir J. Lubbock, that a question as to the frequency of cousin-marriages in England should be inserted in the Census Act of 1871, we bestowed graver consideration upon his labours. These labours have led the author to the sound conclusion that consanguine marriages are innocuous, although the route by which he arrives at this opinion is somewhat adventurous and circuitous. By consulting the 'Registrar-General's Report for 1873' Mr. Darwin finds that 275,450 persons are named, that of these the number of persons to one surname is 8·4. His next step was to count the number of marriages in that redoubtable chronicle, the 'Pall Mall Gazette,' from 1869 to 1873, amounting to 18,528, of which 1·25 were same-name marriages. This curious calculation shows that every seventy-third person is a Smith, every seventy-sixth a

Jones, every 115th a Williams, every 148th a Taylor, and so on. Next it appears that in one marriage in seventy-three one of the parties will be a Smith, and if there were no cause which tended to make persons of the same surname marry there would be one in seventy-three, or 5329 marriages in which both parties were Smiths. Therefore the probability of a Smith-Smith marriage *due to mere chance* is $\frac{1}{5329}$, and so proportionally with the other surnames. Mr. Darwin next examines 700 pages of Burke's 'Landed Gentry,' marking every case where the marriage was same name, and endeavoured to ascertain from the Pedigree all instances in which marriages of persons of the same name were between first cousins. This search proved that of 9549 seventy-two were same-name first-cousin marriages, and seventy-two were same-name marriages not between first cousins, all precautions being taken to exclude double returns. This gives the percentage of same-name marriages as 1·5. When all authorities as to the English and Irish peerage and gentry are consulted there appear to be ninety out of 11,538 or 0·78 per cent. of same-name first-cousin marriages. In reply to circulars addressed to the upper middle and upper classes, it appears that of 3663 marriages there were of first-cousin marriages 125, the percentage of first-cousin marriages being 3·41, and of same-name marriages, whether cousin or not, 1·38 It is necessary to note that only 181 of 800 applications were responded to. We do not enter upon the statistics of the proportions between same-name cousin marriages and not same-name cousin marriages, as the author confesses that his sources of information were inadequate, but he conjectures that same-name first-cousin marriages are to different-name first-cousin marriages as one to four. Candidly enough he applies the terms "perplexing," "discrepant," and "beyond his powers," to his arithmetical calculations, but seems somewhat confident that in London they are $1\frac{1}{2}$ per cent., in urban districts 2 per cent., in rural districts $2\frac{1}{4}$ per cent., in the middle and upper classes $3\frac{1}{2}$ per cent. It does not seem necessary to explain at length the process by which the author measures and weighs the fifty and 150 commonest names inscribed upon cardboard, or arrives at the conclusion "that in England and Wales about one marriage in a thousand takes place in which the parties are of the same surname and have been uninfluenced by any relationship between them bringing them together." His next advance is to apply to the superintendents of nineteen asylums as to the cousinship of the parents of the insane under their charge. The results were that of 18,000 inmates, as to whose antecedents some information could be obtained, about 181 were reported to have been the issue of cousin marriages; but it is

almost needless to state that the returns were defective, or, where complete, utterly worthless. His researches as to the consanguinity of patients in hospitals for other diseases, and labouring under blindness, deaf-mutism, &c., were equally unproductive of reliable facts; and when treating of the literature of the subject, and quoting the observations of Professor Mantegazza on sterility as a consequence of marriages between kindred, he argues that this author was not entitled to infer the infertility of such connections, as we are ignorant of the proportion of barren marriages to the general population. We admire the ingenuity and industry of Mr. Darwin, and the great honesty which pervades his researches, but cannot accept these as important contributions to our knowledge, except in so far that the weight of his opinion swells the general concensus as to the innocuousness of consanguine marriages. This is the appropriate place to remark that a similar attempt has been made in France to determine the proportion of marriages between cousins from official documents, and that in seven years, 1853—1859, of 2,020,224 marriages 17,872 were in this degree of relationship, but that the accuracy of this estimate has been impugned, if not altogether invalidated.

At so many points do the observations of Dr. Nathan Allen touch this subject, that this appears the proper stage for their consideration. They are worthy of investigation upon other grounds. They embrace what the statisticians have unwittingly omitted or purposely ignored; they describe many of the causes and circumstances which must, in a greater degree than mere relationship, influence fecundity, sterility, and the production of healthy offspring, without which, in fact, no exhaustive or comprehensive estimate of the factors of degeneracy can be obtained; and they supply a practical illustration of the speculations as to the perpetuation of races from an unexpected quarter. The attention of the author has been directed to this inquiry by the rapid decrease of the inhabitants of New England, which, should it proceed at the same rate for another hundred years, will in all probability extinguish the descendants of the sturdy and stalwart pilgrim fathers. But while the representatives of the original settlers are disappearing, their places are occupied by German, Irish, Canadian emigrants, or their descendants, so that the gross population suffers no diminution. The loss sustained is, consequently, in the quality or character, rather than in the numbers, of the present occupants of the soil. The early public records show that the first settlers had families of seven to nine individuals, but this prolificness has gradually declined, and the equipoise in the population has been maintained by the fertility of other classes more recently added to

the community. At present it is calculated that the families of the genuine New Englander do not exceed four, and may not be much over three, in number; and although sources of error may arise in some of the census returns, including all inhabitants without distinction; in certain places where the native has been separated from the foreign element, it would appear that during the last century the birth-rate and death-rate have been gradually approximating among the pure American race, and that in certain towns, peopled almost exclusively by this class, the latter exceeds the former. This discouraging fact is not altogether explicable by any special law of unproductiveness, or by the practice of abortion known to exist to a certain extent, and suspected to be resorted to for the very purpose of restricting the number of children, and thus diminishing the cares and toil of maternity, and an elucidation of the difficulty must be sought for in the constitution, the habits, the education, or the lack of physical education of the infertile classes. That a different rate of propagation is characteristic of different races, or of the same race living under different circumstances, has been demonstrated. That luxury, licentiousness, intemperance, and the antagonistic conditions of frugality, virtue, and asceticism, exercise, or may exercise, an important influence over the rate of fecundity, has been admitted; but in all such investigations the state of the organization and of the functions of the parents have been singularly overlooked, and information sought for from remote or collateral sources. The author conceives, and rightly conceives, that the health of the progenitors determines, more than any other circumstance, the numbers, the viability, and the state and strength of the organs and all the functions, mental and physical, of the issue. This truth, or law of propagation, as he styles it, he formulises in the words, "a perfect standard of organization," or "perfectionism of structure and harmony of function," or, in other words, that each organ in the human body should be perfect in structure, and that each should perform its legitimate functions in harmony with others. It would be difficult to define or to describe this standard or maximum health point, or to discover examples of such "perfectionism," even amongst the most finely formed and robustly developed members of rural and unvitiated communities, or amongst those who have passed the rubicon created by insurance companies, military boards, or wherever inquisition is instituted as to "good lives." It is perfectly clear that a mature and undiseased stock is required for the production of healthy descendants. In this sense the effect of hereditary tendencies may be admitted, as, while we may refuse to believe in the transmission of a specific taint, it is obvious that the feeble, the

exhausted, the consumptive, the syphilitic, must be less capable of procreating at all, and of producing strong and uninfected progeny, than the virile and vigorous. In a similar acceptation propinquity may be held to act by intensifying whatever may be most prominent or potent for good or for evil in the constitution of the ascendants, and barrenness may thus depend, not upon impotence created by kinship, but by weakness and disease. It would appear that states of the system much less important than positive or structural diseases, whether innate or communicated, affect or limit reproduction, as, for instance, changes in the balance of nutrition, as in corpulency—the relative activity of different organs, as where intelligence is highly cultivated by intense study, where the imagination and emotions are excited by literature and refinement at the expense of muscular and digestive power, and where all the appliances of modern civilisation are employed in stimulating and thus exhausting the nervous system. As a corollary to this proposition it may be mentioned that giants and dwarfs are unprolific, and that persons of expanded or contracted mind have few descendants. Excess in the gratification of the propensities is visited by a similar retribution, and the vicious, the dissolute, the intemperate, are generally the last of their line. Such results, as well as the deterioration and waning proportions of the original American race, may ensue from adventitious circumstances, or others not included in the copious catalogue supplied by De A——, who, while defending his countrywomen from the imputation that they are indisposed to nurse their children and to undertake the attendant fatigues and responsibility, acknowledges, in the first place, that not more than one half of these mothers are supposed to be able to act in this capacity, or to furnish nourishment to their infants, and, in the second place, that this inability depends upon the imperfect development "of the lymphatic and sanguine temperament" and of the mammary glands. This defect he attributes to modes of dressing, tight-lacing, to the neglect of physical training and exercise, and generally to the influence of a vicious education and of an artificial state of society. He holds that a decided change has taken place in the organization of the female sex in New England during the past century. Formerly there was more muscle, a larger frame, greater fulness of form, and a better development of all those organs that are "classed under the sanguine and lymphatic temperaments." Contemporaneously there was less predominance of the brain, and the nervous system was less excited and over-taxed. This picture now represents the German, English, and Irish intruders, but not the descendants of the original colonists of New England. In addition to such direct

agency, the recourse to artificial food, the disregard of all physiological experience as to clothing, ventilation, &c., must lead to a large increase in the mortality of the young, and may, so far, explain the startling announcement that one third of all children in this portion of the United States die before they reach five years of age. It is marvellous that either by accident or design the natural prevention or remedy for such disastrous social evils should not have been found in the admixture of the two sections of the inhabitants, of those who destroy and those who perpetuate their kind, of the bold and brawny adventurer with his feeble and exhausted predecessors; but it can be easily understood that, even in a country where there is a dead level of rank, and where the acquisition of wealth gives a patent of nobility, great gulfs may separate the old peoples from the new, the rising from those who have risen, the lords of the soil from the sons of toil, even those of higher physical organization from those of weaker or worn-out powers. It was said by a great statesman, of the English, that were not the nobility to marry their cook or laundress in every second or third generation the aristocracy would die out. We would dispute the inference, but commend the practice. Such crossings are calculated to break down that innateness of type which imparts sameness or similarity of qualities to families, and, if the selection be judicious, they would secure a transfusion of new moral pabulum, an interchange of important faculties in which the respective parties may be deficient, of new habits of thought, feeling, action, and if, as some theorists assert, the brain and nervous system of one parent and the nutritive and muscular organs of the other be reproduced, the foundation of a new and more perfect race may be begun. When physiology is universally understood and applied, and when medical men become, as Dr. Allen thinks they should, the guides in all the social relations of life, they may assume the function of regulating such selections, of assorting such unions in accordance with the destinies of successive generations as well as of individuals, and of presiding over a new human evolution!

IX.—Veterinary Sanitary Science.[1]

Mr. Fleming's two volumes fill a void long felt in veterinary literature. The subjects discussed in them are of the greatest importance to the health, the wealth, and in a great measure to the prosperity, of our country. Unfortunately veterinary sanitary science is in its

[1] *A Manual of Veterinary Sanitary Science and Police.* By George Fleming. Two vols

infancy in England, no veterinary medical department for the suppression of contagious diseases having been instituted previously to 1865; and it was only when the cattle plague raged that our Government proceeded to form such a service. It is a department which might be of incalculable benefit in the hands of an efficient and energetic administration. The many papers which have been written upon this subject, by being scattered throughout numerous periodicals and pamphlets, are consequently difficult of access, so that the author has done good service for his fellow-practitioners in issuing this the first complete work in the English language which fully describes this important branch of veterinary medicine. He has shown considerable industry in collecting and arranging the results of the most recent labours of the best Continental veterinary authorities, and has spared no trouble in laying before us in a very clear manner the scientific aspect of veterinary medicine, viz. the nature and prevention of sporadic, enzootic, and epizootic maladies, and their close relation to the diseases of the human subject. To the medical practitioner the most interesting part of the work, and by far the most important, is that treating of the transmission of contagious diseases from the lower animals to man, by contact, by inoculation, and by means of diseased meat and milk.

These volumes strengthen us in the belief, which we have long had, that the diseases of man and of the lower animals are much more intimately connected than many authorities seem to suppose. There is every opportunity of thoroughly investigating by numerous and varied experiments the diseases of the lower animals; and in the case of epizootics, by such a course we are confident that, with such workers as Chauveau, Sanderson, and Klein, much light will be thrown upon the nature of epidemics.

In considering the history of the importation of foreign stock into England we find that the prices of home stock remained unaltered; that there was no direct advantage to the public; and that ever since 1839 we have had outbreak after outbreak of contagious diseases which have destroyed our prime herds and flocks. The result of this is being felt in the present high price of butchers' meat. In fact, the introduction of foreign stock has introduced foreign diseases, which have destroyed more of our home stock than the actual value of all the wretched foreign animals imported into the country. Mr. Clare Sewell Read, M.P., stated in the House of Commons that, according to the official returns of the Board of Trade, the total home supply in towns for 1867, '68, '69, and '70, of butchers' meat was 91¼ per cent., as against 4¼ per cent. live animals and 4½ per cent. of dead meat from foreign countries. If we take into consideration the amount of butchers' meat consumed in the agricultural districts, it will increase our home percentage considerably. From a consideration of the foregoing it appears very unjust that the British

farmer should risk 92 per cent. of his prime stock for the alleged benefit of receiving 4 per cent. of scurvy foreigners teeming with transmissible diseases. So that what the public gain in butchers' meat by foreign importation is represented by a cypher, and what the public do purchase of home-fed stock is increased fourpence per pound. We are not protectionists; we do not object to the introduction of foreign animals to provide food for the rapidly increasing population, but we most decidedly object to the importation of diseases that are detrimental to the interests of the country. Surely the public should be protected in this matter. Free trade in disease cannot bring anything but ruin to health and prosperity. In order to give some idea of this pecuniary sacrifice we quote the following statement from Mr. Fleming's first volume (pages 14 and 15):

"Up to 1869, for the thirty years that had elapsed since the introduction of the two contagious maladies—foot-and-mouth disease and bovine pleuro-pneumonia—it was estimated that the loss from these alone amounted to 5,549,780 head of cattle, roughly valued at £83,616,854. This is, of course, irrespective of the losses from cattle plague. There cannot be a doubt that the same rate of loss has continued, if it has not largely increased, since that period. In 1872, for instance from one malady only—foot-and-mouth disease—it is calculated that the money loss in Britain must have amounted to £13,000,000; but some authorities are of opinion that this is even under-estimated. In Ireland, for the same year, 229,570 cattle were reported by the police as affected with the disease, but this is undoubtedly only a tithe of the actual number, as a declaration of its existence is the exception, not the rule. Nevertheless, if we estimate the loss on each animal reported at £2 (though it may be nearer £4), we have £441,140 to be added to the above sum as the pecuniary loss incurred in the three kingdoms from the existence of one preventible malady only. The damage inflicted by contagious pleuro-pneumonia is probably not much less, as it is always prevalent, whereas the other is more diffused at some seasons than others."

The loss by cattle plague amounted to about £8,000,000.

According to our own investigations into the loss of stock by preventible diseases of foreign origin it amounts to £11,904 per day, and as nearly as possible to £4,000,000 a year, so that in our opinion Mr. Fleming's statistics are most decidedly below the mark.

This is not as it should be. Owing to our insular position, there should be no difficulty in preventing the introduction of diseases indigenous to foreign soils. We hope the time is not far distant when State medicine will fully realise the importance of veterinary sanitary science, and assist in preventing the transmission of deadly plagues from the lower animals to already sufficiently afflicted humanity.

Mr. Fleming's work is divided into four parts. The first section em-

braces a consideration of the nature and causes of sporadic, enzootic, and epizootic diseases, and the influence of traffic on animal plagues. The causes are treated in a very exhaustive manner. The chapters on the "effects of locality" and the "influence of food" in the production of disease will be found most interesting, and will repay careful perusal. Those treating of infection and contagion contain the latest views upon this important subject, the labours of Chauveau, Sanderson, Budd, Beale, and Richardson being clearly discussed.

The results of experiments on the "infection radius," a most important subject for veterinarians, are given. The "elaboration of disease germs," the "mode of access of disease germs," and the "vitality of disease germs," are all ably dealt with in distinct sections. An epitome of the researches of Davaine, Chauveau, and others on "septic infection," cannot fail to be interesting to the profession.

In part second is discussed the prevention and suppression of epizootic and contagious diseases. The value of veterinary sanitary science is fully set forth by the author; and it may be as well to give an extract showing the utility of such a science. He says—

"It must be remembered that the majority of the maladies which come under the cognizance of veterinary sanitary science more or less directly affect every branch of agriculture, and that the damage they cause is not limited to the immediate pecuniary loss and inconvenience attending the inefficiency or death of those affected, but extends to the breeding and multiplication of animals, embarrasses one or more departments of commerce, and generally injures, to a more or less considerable extent, the well-being of mankind. Not only are such diseases formidable by the damage they inflict, but some of them are most serious from the pernicious influence they may exercise on the public health, either by their transmission to mankind by contact or accidental inoculation, or by the use of the flesh or products of the diseased animals as food. Some of the most dreadful and fatal maladies are thus occasioned."

"The maxim that 'prevention is a thousand times better than cure' is founded on the experience—some of it of a very painful kind—of many years; indeed, its truthfulness appears to have been recognised from the very earliest times, though the lesson it inculcates has only too frequently been forgotten. Many of the spreading diseases of animals are not amenable to medical treatment, so that the expense and trouble incurred are completely thrown away. Meanwhile the contagia may be so virulent and subtile that they are continually and widely spread through attempts to cure, and, if the diseases are very fatal, the loss incurred may be enormous and distressing. We have but to remember what happened in Britain during the reign of the cattle plague, in 1865 and 1866. And even if easily remedied and not very fatal, but yet highly contagious, their treatment must be attended

with expense and inconvenience; while their widespread existence, and the loss of service, condition, and productiveness of the animals (to say nothing of the suffering they experience) may render an easily-preventible disease a heavy calamity. We need but instance the so-called "foot-and-mouth" disease. This is, therefore, neither a scientific, a rational, nor a profitable occupation; and Science is unworthy of the name if it neglects preventive measures, even in ordinary cases of disease, and bungles over a useless remedy, or consumes half the value of the creature it attempts to cure, when it might at scarcely if at any cost have maintained it in health."

"Deeply impressed with these facts, the governments of almost every European country pretending to any degree of civilisation have for years been wisely and carefully studying the subject of these diseases, with a view to their prevention and suppression. By several of them laws have been judiciously framed, a veterinary sanitary department of service under government auspices and control has been organized, and carrying out of the preventive measures has been committed to its care; the whole scheme of organization being chiefly founded on the assistance to be derived from the well-educated and thoroughly trained veterinarians, on whom must always devolve the most important and responsible share of the duty in preventing or arresting the spread of destructive diseases. A word from this department, and the machinery of a vigilant government, careful in protecting its subjects from loss, is immediately put in movement; and in this way disastrous consequences are averted, almost without effort, and at a minimum cost. Not only is this organization invaluable in this direction, but it is found to be equally valuable in other ways—such as maintaining the necessary inspection of slaughter-houses; ensuring that the supply of animal food is of healthy and proper quality; keeping a watch on the movements of animals throughout the country with regard to their sanitary condition, and particularly with regard to the existence of contagious diseases; affording instruction in contagious diseases, hygiène, &c., to agriculturists and others; and the drawing up of reports on the health of animals, in which are contained suggestions for their improvement and better management."

The general and suppressive measures advocated by the author are most excellent. They have the merit of being thorough; but, unfortunately, they are too Continental, or, if we may use the term, un-English—being so stringent, smelling strongly of the lands of conscription, that they would not be tolerated for a day by Englishmen, and, in our opinion, must therefore remain a dead letter. But what Mr. Fleming recommends is not lost upon us, as he has set forth the most successful Continental systems of suppressing contagious and infectious maladies; and from it we may select a more simple system which might be enforced in this country, and in a few years would completely eradicate such diseases from our land. We must strike at the root of the evil by preventing the introduction of foreign diseases. This can be done in one of two ways—either stop the importation of all live stock into this country and

let it be sent as dead meat, which plan is adopted at present with the prime Scotch beef from Aberdeenshire to the London markets; or, kill all animals at the port of debarkation. This last plan would not be so thorough as the former, but either course would prevent the continued introduction of the virus and the diseases would naturally die out. In the case of isolated outbreaks fine or imprisonment should be the penalty for knowingly keeping an animal suffering from an infectious or contagious disease. Inspectors should be appointed to different districts with full power to kill, and isolate, the apparently healthy stock, and disinfect premises, as circumstances may require. Inspectors should be independent of private practice. In the case of pleuro-pneumonia they should have power to inoculate all cattle that have been near the affected herd. Compensation to the extent of three fourths should be allowed. If these means were carried out promptly and thoroughly it would soon put a stop to such diseases, provided the importation of the fresh virus be prevented.

A word about the inspection of stock at public markets, in steamboats, &c. We are compelled to say, from our experience of such duties, that it is of doubtful utility, as inspectors cannot possibly recognise disease during its incubative stages; for instance in the case of pleuro-pneumonia the period is forty days; in cattle plague six days, and in epizootic aphtha from three to five days. All that the inspector can do is to reject the number of animals actually showing the disease; but it is evident that the pet system of the Privy Council Office (veterinary) can be no protection to our home stocks.

In the chapters on "disinfection and disinfectants" veterinarians will find all that is necessary for their guidance.

Part third, the special contagious diseases are discussed, such as cattle plague, contagious pleuro-pneumonia in cattle, epizootic aphtha (foot-and-mouth disease), glanders, farcy, strongles, influenza, variolous fevers, anthrax and anthracoid diseases, splenic or Texas fever in cattle, fowl cholera, rabies, distemper in dogs, venereal diseases of solipeds, tuberculosis of cattle, and other important contagious diseases. In our humble opinion this is by far the best portion of the work. The author gives a full description of their history, causes, symptoms, terminations, and treatment. The pathological anatomy is also admirably described, and can be fully depended upon—which is unlike most veterinary works, as they are at least thirty years behind in this important branch of science. In the article on contagious pleuro-pneumonia inoculation as a preventive is fully discussed, and we would urge veterinarians to study and practise the hints thrown out. The chapter on epizootic aphtha (foot-and-mouth disease) is important to medical practitioners, as it is a disease that is very frequently transmitted to the human subject. We have long been convinced from our obser-

vations that during an extensive outbreak of this disease (provided the cow's udder be affected) it spreads to mankind through the milk, although it may be and is transmitted in other ways. Cow's milk is now more used than ever in the alimentation of infants, and we are assured that it becomes an important factor in the production of intestinal and eruptive diseases in children. This poison, if once introduced into the body, has, without doubt, a tendency to a fatal termination in all young animals. The bad effects of such milk upon man have been observed by Hartwig, Villian, Gamgee, Gilmot, Watson, Dr. Hislop, Dr. Thom, and Dr. Nichol, Boston, U.S., and many others we could mention. As the disease is at present spreading over the length and breadth of the country, we trust, should cases occur, medical men will report them to the medical papers.

Glanders, another most terrible disease and one easily communicable to man, is discussed. An idea of our danger from this source may be gleaned from the fact that the annual loss is estimated to be over £20,000.

Tuberculosis in cattle is the subject of a suggestive paper. Unfortunately this has become a very common disease among dairy cows. One extract will indicate the drift of the paper. "Gerlach and others have demonstrated that the milk of tuberculous cattle will produce phthisis in creatures fed with it."

Part fourth embraces a consideration of the inspection of slaughter-houses, meat, milk, and knackers' yards. This part of the work will be found most useful to officers of health, as the hints given may assist them in their important duties.

The merits of this excellent work are so unquestionable that we have little hesitation in pointing out what we suppose might be improvements. It is to be regretted that the auther did not issue the work in two separate books, the first on veterinary sanitary science and police proper, the other as a treatise on the diseases of the lower animals communicable to mankind. As a text-book for veterinary students and agriculturists the work would admit of condensation, and would be improved by the absence of such technicalities as occision (page 249), pro drome (page 325), bossilated, and others of a like nature. The absence of many of the woodcuts would be no great loss to the work, as some of them are really bad. The effects of sudden change of food should be treated at greater length, as it is one of the most prolific causes of blood disorders in this country. The same remarks apply to the "effects of pastures." A chapter on the effects of immature food is much wanted, as it frequently gives rise to intestinal disorders and even symptoms of poisoning.

In conclusion we have no hesitation in saying that Mr. Fleming's book is one of the best works on veterinary medicine in the English language.

X.—Clay's Obstetric Surgery.[1]

We may credit the statement of the author in the commencement of the preface—"The fact that a third edition of this 'Manual of Obstetric Surgery' has been called for is sufficient proof of its general utility." The present volume, he asserts, has been carefully revised, considerably enlarged, and contains additional illustrations. We may add further that it is printed in a clear, readable type; is of a handy size, not too large for the pocket; and that the illustrations are neat, though not very numerous.

In dealing with the subject of operation the author includes not only those cases where the surgeon's knife, ligature, or caustic are the chief agents, but also those which require mechanical and manual aid, as versions and operations with the forceps, vectis, blunt hook, &c.

The value of the book when it originally appeared was doubtless great, and possibly has done much to allay the anxiety of many a young practitioner who had treated the subject of midwifery with far too much indifference when walking the hospitals, or had enjoyed little opportunity of learning it practically.

What we shall principally endeavour to examine in the present notice will be the question of whether the work has kept pace with the times—whether, in fact, the information and instructions are "up to date." On turning to the subject of (induction of) abortion, the author states he has no confidence in medicines to procure abortion. "The only certain means is by mechanically rupturing the membranes, and then directly destroying the vitality of the embryo in utero" (p. 14).

As a natural sequence to this we are quite prepared to read further on, p. 16—"There are dangers to be feared arising from the operations of a highly responsible and not unfrequently fatal character, such as hæmorrhage, metritis, and peritonitis."

Need we say that in the early months of utero-gestation, the cervix being closed and the membranes high up, it must be difficult to puncture these latter without a risk of interfering with the integrity of the cervix, and of thus setting up serious or even fatal mischief, as so frequently happens in cases of criminal abortion? This plan of rupturing the membranes, excepting in special cases of hæmorrhage, is condemned by nearly all modern writers on the subject, as being unscientific in theory and dangerous in practice. It is not *inducing* the uterus to throw off its contents, but aggravating it, so to speak, into open hostility. It is compelling the uterus to expel the ovum in a directly inverse method to that adopted by nature.

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[1] *The Complete Handbook of Obstetric Surgery.* By CHARLES CLAY, M.D. Third edition. 1874.

The plan suggested by Barnes, and now very generally practised, of *inducing* premature labour or abortion is, by passing a bougie or catheter, and allowing it to remain between the membranes and the interior of the uterus, accelerating by dilating the cervix, and not puncturing the membranes, until the os is sufficiently dilated to allow of the passage of the ovum, and not even then unless requisite. The author has, of course, a perfect right to his own opinion; but it would have been better had the more modern method been mentioned as well, together with the diminution of risk, and if he had not confined himself to the statement,—"The only certain means is by mechanically rupturing the membranes."

In speaking of Cæsarean section the author asserts that it may be done "a full hour after the death of the parent with a chance of success," p. 45. Although we do not doubt this may have happened, it is more than probable that "dead more than hour," owing to the unintentional exaggeration of the friends, would more correctly have been stated at half if not a third of that period. In any case, if the doctor be present, we would advise his operating at once if he intends to do so at all.

The details of the operation are clearly given, and evidently are the result of practical experience.

Puerperal convulsions are probably the most trying complication of labour that a young practitioner can encounter; but even this fact will scarcely warrant the indiscriminate use of the lancet. "Bleed freely to thirty or forty ounces," p. 12, is generally at the present time commuted to the prescription to administer chloroform or exhibit chloral; and as to shaving the head, unless in very exceptional cases, we doubt, if the patient recovered, whether she would ever forgive the doctor. After some considerable experience in these cases, we do not hesitate to say that shaving the head and bleeding to thirty or forty ounces are expedients now very seldom requisite.

The experience of Charpentier (and many others) goes far to prove the great value of chloral and chloroform in these cases. "The mortality in cases treated by bleeding was thirty-five per cent., whereas that where anæsthetics were used was eleven per cent.," are the statistics given in his late thesis.

Lancing the gums is hardly an obstetric operation, however necessary it may be for the student to know when to do it, and what to expect from it. Respecting embryotomy, the rule laid down is, that "it is justifiable when there has been strong labour for some hours (about five or six) and no advance;" it is also admittedly justifiable as soon as ever the uterus is sufficiently dilated to allow of the passage of the fœtus, provided the capacity of the pelvis is such that it is impossible for a living child to be extruded.

"The perforator and crotchet are the only instruments necessary," so says Mr. Clay; we beg to differ very materially. The cephalotribe

or craniotomy forceps are absolutely necessary to effect delivery with safety to the mother, where the brim is so contracted, or the capacity of the pelvis so interfered with by tumour, as to preclude the passage of a living child. The risk is considerably less than where extraction is attempted by the crotchet alone.

We had thought that the days of pewter syringes had long past, yet we find them recommended by Dr. Clay, as if the modern improvements in vulcanized india rubber, which in every respect are far better, did not exist. The form of syringe figured p. 78 is the one best adapted to produce cramp in the hand; the oval shape is far better.

On "evolution—expulsion spontaneous:"—"When the body of the child is fairly engaged in the pelvis and the arm presenting," wait for natural efforts; and after watching these closely, "if for some time no advance is observed I advise traction." "Turning and evisceration are altogether discountenanced." Can anything be plainer than this? We quote the *ipsissima verba* that we may not be taxed with misrepresentation.

Further on in the work, under arm presentation, the author tells us spontaneous evolution before the child is engaged in the brim is hardly possible; after that, impossible.

The chapter containing the former instruction and opinions needs re-writing; it is difficult to reconcile the two statements. In prolapsus of the cord nothing is said of the postural treatment, a very important omission.

Progressive shortening of the cervix uteri in advancing pregnancy is figured and described according to the old views, and the results of modern observation left unnoticed.

In ulceration of the os or cervix uteri the method suggested of squirting injections into the uterus with a small glass syringe, without the aid of a speculum, is, to say the least, injurious, if not dangerous.

In speaking of fistulæ—vesico-vaginal—the author talks of "incurable cases," "surgery yet at fault," and "great room left for improvement." Surely these remarks must have been written many years ago, and are not intended to apply to the experience of to-day. Thomas, in his excellent treatise on diseases of women, endorses the opinion of Marion Sims, that "every case is curable when the operation is practicable." "Success is the rule, failure the exception."

The indications for the employment of forceps are thus summarised:

"If twenty-four hours after rupture of membranes no advance has been made for the last four hours; if exhaustion manifested by quick pulse; cessation of pains; greenish discharges; unpleasant smell; anxiety of countenance; hurried breathing; tongue coated; vomiting; shivering; coldness; and muttering delirium; *then la-*

bour must be terminated." The italics are our own. The student who relies upon this advice, who waits *till then*, and only then thinks artificial assistance requisite, will, we fear,—to say the least,—have small success in practice.

Speaking generally, we would say, if an hour after rupture of the membranes—*not* twenty-four—no advance has been made, forceps or other assistance should at once be resorted to. There is no object in waiting for hours, and often, as we have seen, whole nights and days, watching a patient uselessly expending her strength, endeavouring to propel her offspring through an aperture too small to allow of its passing, or with force too feeble to accomplish the object. Modern midwifery prides itself upon timely assistance. Dr. Hamilton, of Falkirk, has proved that the danger of forceps consists only in delaying their application, not in their employment; and we can thoroughly endorse his views. How many cases of pelvic "cellulitis," vesico-vaginal fistulæ, and pelvic peritonitis are due to this inane inactivity! and when the would-be teachers of our youth advise us thus to wait, who can wonder at the distressing revelations of the coroners' court that from time to time are made to us, or wonder that the public proceed against practitioners for malpraxis!

The author started with the promise of "saying all that is really necessary, but no more;" nevertheless he gives us no less than five-and-twenty pages on the subject of gestation, entering fully into details of the time taken by rabbits and mares, sows and cows, details which, though doubtless interesting, are quite out of place in the present handbook. The gist of the whole matter seems to be that the duration of pregnancy is materially influenced by the respective ages of the parents.

Concerning hæmorrhage with hydatids, we differ entirely from the author as to manual help not being necessary before the seventh month. If the case be proved to be one of hydatidiform degeneration of the villi of the chorion, whether it be at the fourth or fifth month, the sooner the uterus is emptied of its contents the better.

As to accidental hæmorrhage, few will now be found who bleed as suggested, or who give acetate of lead and opium.

In placenta prævia, in place of turning and delivering as soon as the os will permit (p. 152), or detaching the placenta *entirely*, most authorities now content themselves with rupturing the membranes, peeling the placenta *partially*, not entirely, in concentric rings, from the cervix, applying the tampon, if necessary, and waiting on nature for further indications.

No mention is made of the injection of the solution of perchloride of iron in cases of post-partum hæmorrhage; it should at least have been referred to, even if the author does not agree with its employment.

In cases of retained placenta, "if the hæmorrhage be trifling,

delay extraction." This we are told in one sentence, and yet further on we find it stated that "the patient is not safe until it is removed," and that "delay gives rise to apprehension and involves the character of the attendant."

In hour-glass contraction with retained placenta, "if the symptoms are not urgent, give an anodyne and wait." A little gentle pressure externally with the finger, introduced in form of a cone, and the placenta extracted, will generally overcome the irregular contraction at once without any need of opium.

The practical reference table for treatment of hæmorrhage is very clearly drawn up, but "lead and opium" would be as well expunged.

In the treatment of hæmorrhoids after labour the plan of dilating the sphincter ani forcibly immediately after the expulsion of the child, while the patient is still under the influence of chloroform, and returning the piles, is not alluded to. This is a very successful expedient.

For harelip he suggests waiting until the second or third *year* of life before operating; most modern authorities resort to it when the child is four to six *weeks* old—an important difference.

In tedious or difficult labour, "where inertia of the uterus or deficient contraction attends delicate, exhausted primiparæ," he believes the great remedy to be opium—a pill of two, or two and a half grains, or one drachm of tincture, so as at once to suspend uterine action and obtain sleep. Chloroform is very properly suggested, ergot not being forgotten, but we should prefer seeing advised an earlier resort to the employment of forceps. Venesection is still "the only remedy" for rigidity of the os;—"avoid artificial dilatation"—"propriety of incision of os very questionable;" such are his views. We beg to differ; incision is a very valuable resource in properly selected cases, dilatation, artificially, being also very useful.

For extra-uterine pregnancy the "treatment is pretty nearly the same in all the forms" (p. 244). "The rupture of the cyst should be retarded as long as possible." No reference is made to electrolysis, performed per vaginam, as advocated by Thomas, or to gastrotomy, if this be impracticable.

In speaking of version, cephalic and podalic, the ordinary internal method is given, and Wigand's plan of external abdominal manipulations alluded to; but no mention is made of the method adopted by Dr. Braxton Hicks of conjoined manipulation, which is by far the better one.

The time has come to speak plainly; and with no personal feeling whatever, but merely on principle, we feel bound to advocate a more rational system of obstetric surgery than that suggested by the handbook now under review. The author has scarcely fulfilled the

promise of his preface in making the work a trustworthy guide to young practitioners.

Coming as it does with the weight of Dr. Clay's authority, it is all the more important that the information should be sound, and in accordance with the recognised views of our chief professors and teachers. We have no hesitation in saying this is not the case, and, with every respect for the author, we suggest a reconsideration of some of the important points we have above referred to. For the sake of his own reputation, for that of his readers who may be guided by the practice recommended in its pages, and for that of the well-being of parturient women, it is incumbent on Dr. Clay to revise the 'careful revision' of his matter, for which he has taken credit in the preface to the present treatise on obstetric surgery. It is doubtless a legitimate satisfaction to an author to have a third edition of his work called for; but it is, at the same time, fraught with the danger of intermitting the care and diligence which have in the first instance won for it success.

Bibliographical Record.

Contributions to the Mechanism of Natural and Morbid Parturition.[1]—In this volume Dr. Matthews Duncan presents in a collected form many of the contributions whereby he has enriched the literature of scientific obstetrics, and especially that portion which deals with the mechanism of parturition, a subject second to none in the science of obstetrics, and upon which Dr. Matthews Duncan's writings stand in the very foremost rank.

Dr. Duncan does not profess to give us much that is new in this volume, but by publishing his contributions in a compact and connected form he has laid all who desire the progress of medicine, and especially of the obstetric branch, under obligation. Yet though there is but little that is new *quoad* the author; there is a great deal of solid scientific material which it becomes a duty incumbent on all who practise, and pre-eminently those who are privileged to teach, midwifery to make themselves acquainted with. In Germany the subjects considered in this volume have long attracted more attention, and have been more written upon, than in this country, but the able writings of our own countrymen, including the author, Dr. Barnes, Dr. Leishman, and others, are in no degree inferior, to say the very least, to those of our German confrères. There is no necessity to go to a foreign source for information and instruction while we have such able productions in English as those of the writers just alluded to.

Unfortunately the length of the course allotted to midwifery in the curriculum of our schools is much too brief to enable the lecturer to enter upon a sufficient exposition of the principles of the mechanism of parturition. The course is so crowded that he must hurry on to other subjects more urgently demanded by the examining boards. Hence the principles on which the science of midwifery is founded are but imperfectly, if at all, comprehended by the majority of students.

We will endeavour to give a critical sketch of the present volume,

[1] *Contributions to the Mechanism of Natural and Morbid Parturition.* By J. Matthews Duncan, M.D., &c. Edinburgh, 1875.

from which we trust our readers will gather a not inaccurate idea of the value of Dr. Duncan's contributions.

The first chapter of the book is appropriately dedicated to a consideration of "The Mode of Progress of the Science of Natural Parturition."

Herein Dr. Duncan seeks to direct attention to the mode in which increasing knowledge of the function of natural parturition has been ripened into a science of this department of medicine. As natural parturition is more common, so, says Dr. Duncan, is this department of science more important than that of morbid labour. To the modern tendency to strain after utility or practical results Dr. Duncan gives a philosophical caution. We should rely, he argues, on practical good coming out of honest scientific labours without always having before us the immediate attainment of so-called practical results. We should not set about the search for gold by digging in the nearest field, but by the study of geology. He sneers rather at what he calls "*mere* practitioners" who have, he says, "flourished since the world began, but have done very little progressive work for the benefit of the race. That has to be slowly elaborated by an humbler sort, the men of science."

Of science Dr. Duncan truly says—"Its achievements are certain, secure, beneficent. They may be diffused over the world without being thereby diluted. They are capable of being expressed in written language, and handed down to every coming age."

After a severe satire on the tendency to run after new "practices," the author points out that William Hunter was the first who satisfactorily completed, to a large extent, the science of parturition in his great work published in 1774. "This grand foundation-stone of obstetrical science," says Dr. Duncan, "still remains securely fixed, unsurpassed in perfection of elaboration, and more admired than when it left his hands."

The great French writer, Levret, comes in for a due meed of praise, and Dr. Duncan remarks that every day shows more and more the correctness of Levret's assertion that parturition is a "natural operation, truly mechanical, susceptible of geometrical demonstration."

The first great advance in scientific midwifery, consisting in a knowledge of topography, measurements, shapes, axes, being achieved, the second great stage, the discovery of the manner in which the fœtus traverses the maternal passages, and the alterations in form thereby produced, remains still incomplete, though much has been done towards it.

The third great department of scientific midwifery is engaged with the forces employed to do the work. This involves problems of much higher complexity than the other two. Here the amount

of labour required is enormous, and the author wisely enjoins all who are ambitious of contributing to the elucidation of the problems involved to "learn to do a small thing well before they try high tasks."

This chapter concludes with a fine peroration on the simplicity of great discoveries when once they see the light, and on the great obscurity in which they were previously enshrouded.

The next chapter is on "Long Delay of Labour after Discharge of the Liquor Amnii." That this accident may occur and the pregnancy continue for some weeks Dr. Duncan distinctly avers, and he gives illustrations of it. It is also proved by the continuance of a pregnancy in which the uterus was tapped by mistake for an ovarian cyst. The sufficiency of the explanation of the continuance of pregnancy depending upon the situation of the rupture being high up is not admitted by Dr. Duncan.

"The Curves of the Developed Genital Passage" is the subject dealt with in the next chapter. The points of special interest in respect of these are very ably discussed. A sufficient analysis of this chapter would occupy too much space, but we would recommend all who profess to undertake the management of labour to study the subject by the light here offered. There is one very important point, however, which we cannot forbear dwelling upon; it has reference to the dilatation of the soft parts which form so large a portion of the third or greatest curve of the genital passage. On this Dr. Duncan says, "There is to be noted, also, in connection with this curve, the inevitable tendency of the force of labour, not merely to distend the perineum, but also to rupture it centrally, to force the presenting part through it—a tendency, the study of which, apart from other considerations, leaves no possible doubt as to the expediency of the practice of supporting the perineum; a practice which can be demonstrated to favour the maintenance of its entirety." This we take to be a distinct protest that the modern practice of abandoning the support of the perineum is wrong. While decidedly of opinion that fussy and unintermittent support of the perineum is objectionable, we agree with Dr. Duncan that, properly applied, the practice is one of extreme value.

Chapter IV is devoted to a consideration of "The Power Exerted in Ordinary Labours." This, the author thinks, may be measured by discovering the tensile strength of the membranes, and many experiments with that object are detailed. No very practical or reliable result appears to have been obtained.

We next meet with a chapter (Chapter V) dealing with "The Greatest Power of Labour exerted in Difficult Cases," which Dr. Duncan points out may in a number of cases be to some extent ascertained by measuring the power exerted by the forceps. Instruments for this purpose have been devised by Joulin, Kristeller, and

others. No very satisfactory conclusions appear to have been arrived at at present, and the subject awaits further investigation.

Next we have a chapter (VI) "On the Power of the Uterus to resist a Bursting Pressure." This has obviously an important bearing on the subject of rupture of the uterus.

The enormous strength of the uterus was fully demonstrated in the experiments made; and this very important conclusion naturally follows—that the uterus is never spontaneously ruptured when it is healthy. This has long been our own opinion, and it is one the importance of which is singularly great at the present time owing to the prosecution of accoucheurs who have had the misfortune to be in attendance upon women to whom this calamity has occurred.

"The Efficient Powers of Parturition" are next considered.

Dr. Duncan concludes that very few of the most powerful labours exert a force of fifty pounds; this he arrives at by measuring the amount of force required to keep back a head just emerging from the vulva. He is here in conflict with the Rev. Dr. Haughton, who, by measuring the bulk and extent of the voluntary and involuntary muscles employed in parturition, arrives at the conclusion that "on an emergency somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner." Dr. Haughton regards the voluntary muscles as supplying the greater part of this power, crediting them with 523 lbs. and the uterus itself with 54.

Dr. Duncan, on the other hand, concludes that the comparatively small figure of 80 lbs. gives the highest power of labour, voluntary and involuntary effort together. The pressure of a quarter of a ton would suffice, says Dr. Duncan, to bray the child to pieces, and would, "if appropriately applied, not only expel the child but also lift up the mother, the accoucheur, and the monthly nurse, all at once."

Chapter VIII is "On the Chief Directions and Extents of Uterine Shrinking; specially at the time of the complete Expulsion of the Contents of the Gravid Uterus." Dr Duncan's method of observation on these points consists in noting the rugæ, ridges or wrinkles, and the sulci, furrows, or grooves occasionally found on the peritoneal surface of the contracted uterus and likewise the cracks or fissures encountered on the same membrane. No satisfactory results, however, are apparent.

"The Tensile Strength of the Fresh Adult Fœtus" is next in order. In some interesting experiments made on fresh fœtuses, the author found that an average weight of 120 lbs. caused decapitation. Before this occurred, however, the spinal cord was injured, the cervical vertebra giving way.

In the following Chapter (X) Dr. Duncan enters into an elaborate

and instructive discussion "On the Pelvic Joints in Parturition." He distinctly states that there is an appreciable amount of normal mobility in them in parturient women, and he demonstrates the mechanism of this. He says, "The movements which occur may be described as consisting in the elevation and depression of the symphysis pubis, the ilia moving upon the sacrum; or if the sacrum be regarded as the moving bone, it describes a nutatory motion upon an imaginary transverse line passing through the second bone," and a diagrammatic representation is given in illustration of this movement.

There is, too, slight mobility of the pubic symphysis. *A propos* of this subject Dr. Duncan remarks that, in his opinion the Sigaultian operation (symphyseotomy) has been too readily abandoned.

"The Obliquity or Lateral Flexion of the Fœtal Head" is considered in Chapter XI. "The object of this paper, is," says Dr. Duncan, "to show that the obliquity, or lateral obliquity, of the fœtal head when passing through the brim of the pelvis, described by Naegele, by some of his predecessors, and by his followers down to the latest authors, does not exist in natural parturition; and that obliquity, or lateral obliquity, of the fœtal head when passing through the outlet of the pelvis, not described by Naegele and his followers, does occur in natural parturition." With a slight qualification this position is supported by another very able writer on the mechanism of parturition, Dr. Leishman. The arguments adduced certainly appear to invalidate Naegele's statements.

"Obliquity or Lateral Flexion at the Outlet" (Chapter XII), on the contrary, is believed to occur both by Duncan and Leishman, and its mechanism is explained in this chapter.

The next Chapter (XIII) is "On the Synclitic Motion of the Fœtal Head"—a condition described by Küncke. The account given hardly admits of a critical analysis consistent with the space at our disposal.

Chapter XIV, "On the Production of Presentation of the Face," is of considerable interest. Dr. Duncan does not admit that the dolichocephalous form of head exerts that amount of influence in the production of face presentation attributed to it by Hecker, of Munich; he explains this occurrence otherwise, as we shall presently see, but he acknowledges that dolichocephaly may be an important factor in the mechanism as he apprehends it. Lateral uterine obliquity and the second position he regards as the chief factors.

The next chapter (XV) need not detain us: it is "On the Caput Succedaneum, the presentation, and their relation in cases where the head comes first." The following one, however, calls for more attention; it is on "The Expulsion of the Placenta" (Chapter XVI). Dr. Duncan first shows the astonishingly erroneous conclusions arrived at by most authors who regard the expulsion

of the placenta as occurring in an "inverted umbrella" fashion: an error of observation which Duncan clearly points out, while he shows the true mechanism of the normal expulsion of this body. Our own close observation of this subject enables us to confirm the statements made by Dr. Duncan, that the placenta presents by its edge, and that it is folded on itself longitudinally.

Chapter XVII is devoted to an allied subject: "The size of Aperture necessary for the passage of the Placenta, and for the passage of the Accoucheur's Hand." This is of interest specially in relation to the treatment of placenta prævia. From experiments made Dr. Duncan found that an aperture fully two inches in diameter was required for the transmission of the uninjured mature placenta. The hand can be passed through an aperture from 2½ to 3 inches in diameter. "It may, therefore, says Dr. Duncan, "be safely asserted that, in the very great majority of cases of placenta prævia, the hand may be passed into the uterus if the placenta can be extracted from it in a satisfactory manner without disruption."

Akin to the foregoing subject is that discussed in the next (XVIII) chapter, "On the Changes undergone by the Cervix Uteri during Labour." During pregnancy the cervix undergoes hypertrophy or enlargement in every direction, but until shortly before labour sets in it usually forms no part of the cavity containing the ovum. After delivery the cervix is elongated, thinned, and softened, being in great contrast with the body of the uterus, which is shortened, thickened, and hardened. The influence of this condition on lacerations of the vagina is briefly considered. We next have a chapter (XIX) on "Increased Length of the Cervix Uteri after Labour." This is stated to be considerable, so much so as to lead some to believe they have to do with a case of hour-glass contraction, when in truth the narrowed point is but the normal contraction of the tissues at the junction of the cervix with the body of the uterus. It is right to say that the conclusions of this chapter are questioned by Professor Isaac E. Taylor, of New York, in the 'American Journal of Obstetrics' for May, 1874.

"The Production of Inverted Uterus" is the subject discussed in Chapter XX. Dr. Duncan enters into an elaborate disquisition on the manner in which this accident is produced. He maintains that there must be paralysis of the whole or a part of the body of the uterus before inversion can be begun. This view Dr. Duncan shows is not altogether a new one, and of its truth there can be but little doubt. The placental site being so commonly the seat of the partial form of paralysis, it seems to us that a very important point of practice flows from this, viz. the desirability of speedy separation of the placenta. We are of opinion that there is very commonly too great delay in the removal of the after-birth, and would say—If the

placenta be separate, remove it; if it be adherent, separate it—both without delay.

Chapter XXI is devoted to "Hæmorrhage during Pregnancy in Cases of Placenta Prævia." This the author states may be caused in four ways:

1. By rupture of a utero-placental vessel at or above the internal os uteri.
2. By rupture of a marginal utero-placental sinus within the area of spontaneous premature detachment. . . . .
3. By partial separation of the placenta from accidental causes, such as a jerk or fall.
4. By partial separation of the placenta, the consequence of uterine pains, producing a small amount of dilatation of the internal os. . . . .

In hæmorrhage from the two first sources there need be no subsequent morbid alteration of the cotyledon or cotyledons; whereas in the two latter there will, if the pregnancy last long enough, be well-known pathological changes—thrombosis and decolorisation, and ultimately atrophy. The author's discussion of the various theories advanced on this subject is too diffuse for condensation, and any analytical criticism worthy of his high reputation would occupy much more space than we could accord to it. We may say, however, that he refutes the ordinarily accepted doctrines, and thinks that much harm has arisen from authors treating unavoidable as quite distinct from accidental hæmorrhage; the fact being that their whole pathology, though not identical, is nearly so.

Allied to the foregoing is the subject dealt with in the next chapter (XXII), viz. "The Spontaneous Separation of the Placenta when it is Prævia." Duncan's contention, illustrated by a variety of arguments, is that the placenta is not attached to the cervical portion of the uterus, the cervix forming no part of the ovum-containing cavity; that, in fact, "a placenta prævia is attached to the body of the uterus above the cervix, and may cover its internal os." Here he is in direct conflict with other distinguished authors. In Dr. Duncan's view separation of the placenta, when it is prævia, is due to expansion of its seat of attachment to the uterus, and not to contraction thereof. "Expansion of the placental insertion," says he, "in the first stage of labour is the distinguishing specialty of placenta prævia, and has detachment as its distinguishingly peculiar result."

Chapter XXIII, "On the Causes of Unavoidable Hæmorrhage during Miscarriage or Labour when the Placenta is Prævia," is a continuation of the important subject of Placenta Prævia. Dr. Duncan does not hesitate to say that "when the placenta is truly prævia—that is, attached when labour begins to the area of spontaneous premature detachment—hæmorrhage is unavoidable." A

laboured discussion of the subject follows, which we do not propose to enter upon.

Chapter XXIV continues the further discussion of this question; it is "On the Sources of Hæmorrhage during Miscarriage or Labour at Full Term in cases of Placenta Prævia." This very interesting subject appears to be still surrounded wlth considerable obscurity. Dr. Duncan somewhat favours the view that the circular sinus of the placenta is a frequent source.

Chapter XXV deals likewise with placenta prævia. It is "On the Mechanism of Arrestment of Hæmorrhage in cases of Placenta Prævia." This is pretty fully discussed, but we do not think we can profitably dwell upon it.

We next come to the appendix of the book, which comprises Dr. Duncan's address in obstetric medicine at the Norwich meeting of the British Medical Association in 1874. Beginning with an able plea for the division of labour, the author next goes on to enforce the importance of method. He then directs attention to the importance of mechanism, the scientific value of which he rightly proclaims. Important as a knowledge of the mechanism of normal parturition is, the writer admits that a knowledge of that of morbid parturition is more eagerly sought after by the bulk of practitioners, but he laments the absence of secure guidance in this matter. Notwithstanding all the able writings and teachings on this important subject we are still, he says, without the knowledge we require. Still more difficult than the foregoing are natural and morbid childbirth.

Dr. Duncan's address has been so recently given to the world, and doubtless has been so widely read, that we content ourselves with this brief mention of it.

In conclusion we may say that this volume of collected papers confirms the general belief that Dr. Duncan deservedly stands in the foremost rank of past or present scientific obstetricians.

Steiner on Children's Diseases.[1]—Fifteen years of uninterrupted activity in the Children's Hospital at Prague have encouraged and given some claim to the author to write this treatise. It professes to be a compendium of children's diseases, a trustworthy guide to the student as well as the practitioner.

The mere fact of a second edition of the work having been called for within three years of its first appearance is a sufficient guarantee of its appreciation by the profession in Germany, and has doubtless influenced the translator in selecting it as worthy of being

[1] *Compendium of Children's Diseases: a Handbook for Practitioners and Students.* By Dr. Johann Steiner. Translated from the second German edition. By Lawson Tait, F.R.C.S. London, 1874.

presented to the profession in England in the language best suited to the wants of practitioners.

The author has arranged his subject under nine divisions: the first comprising the Investigation of Disease; the second, Diseases of the Nervous System; the third, Diseases of the Organs of Respiration; the fourth, Diseases of the Organs of Circulation and of the Lymphatic System; the fifth, Diseases of the Organs of Digestion; the sixth, Diseases of the Urinary and Sexual Organs; the seventh, General Diseases of Nutrition; the eighth, Zymotic Diseases; the ninth, Diseases of the Skin.

Under the first heading, the Investigation of Disease, the author very rightly observes that "the absence of speech, the uncertainty of the communications concerning the subjective disturbances of the conditions of health, the wilfulness, dislikes, fear, and agitation of the child, make an inquiry difficult or even impossible; and this is so much the more if the practitioner does not understand how to win for himself the confidence of the patient."

Under "Diseases of the Nervous System" are included Diseases of the Brain and its Membranes, and of the Spinal Cord and its Membranes. The subject is treated very exhaustively, all the known varieties of nervous disorders being briefly referred to, and the researches of those who have directed attention to the pathology of special lesions being incorporated in the text so as to bring the subject quite up to date.

Epilepsy is grouped among Diseases of the Spinal Cord and its Membranes, though the author in treating of the subject remarks that "post-mortem examinations have revealed so many various essential alterations that the conclusion has been arrived at that *there has been no specific lesion* by which the disease can be accounted for." He regards epilepsy as a common disease of childhood. In speaking of treatment, no mention is made of what measures should be adopted during a fit, nor any restrictions given as to diet, exercise, &c. We fear the practitioner would be somewhat disappointed when referring to this chapter for any hints or indications for treatment; indeed, the whole subject is very cursorily treated.

In the chapter devoted to Diseases of the Organs of Respiration the various conditions met with are duly given. In speaking of catarrh in early life sufficient importance is scarcely accorded to the syphilitic coryza so often met with, and which is too frequently mistaken by the unwary for a mere cold in the head. The advantage of administering mercury through the mother's milk is not alluded to, and the propriety of abstaining from baths in the case of infants, merely because they have idiopathic nasal catarrh, is, in our judgment, of doubtful expediency. The description of croup is evidently drawn from frequent observation of this distressing malady, and is given with clear, minute, and circumstantial detail. The author

does not agree with those who regard ordinary inflammatory croup as infectious, though he believes there can be no doubt that the diphtheritic variety is eminently contagious. He states that 34·6 per cent. of the cases in the Children's Hospital at Prague have been saved by the operation of tracheotomy.

He depends chiefly on inhalations and emetics, the most useful of the former in his experience being lime-water, and ipecacuanha for the latter. Tracheotomy, he observes, constitutes of itself no remedy against the croup, but by giving nature time to bring about more favorable conditions it is often of great importance.

The translator has supplemented a manifest defect in dealing with this subject, by giving an account of the after-treatment required; and, as we should have expected, it is practical and thorough. We regret he has not made the chapter complete by giving the details of the operation itself, for we feel confident it would have been much valued by many a young practitioner.

The subject of foreign bodies in the air-passages is somewhat summarily dismissed; considering the frequency and danger of this accident, a more detailed account would have been acceptable.

Hooping-cough is described at some length. He regards it as an epidemic, neurotic contagious bronchial catarrh, accompanied by spasmodic attacks of coughing. Belladonna is recommended as the drug most likely to be of service. No notice is given of chloral, which in severe cases has been proved to check the severity and lessen the frequency of the paroxysms of coughing, as also to induce sleep and allay the distressing vomiting so frequently met with in this disorder.

In speaking of phthisis, the author supports the tubercular, pneumonic, and bronchitic origin of this malady.

In pleurisy, "when symptoms of suffocation occur from the extreme amount of effusion, the chest must be tapped without loss of time," remarks the author; though, "if the pleurisy should become purulent and the symptoms become unfavorable, thoracentesis must be performed; though very frequently the natural efforts will create an exit" (pp. 173-4). An earlier resort to thoracentesis, as advocated by Bowditch, Playfair, and others, together with the plan of subaqueous drainage, seem justified by recent experience, and are points of great importance in the management of these troublesome cases.

In speaking also of pericarditis with effusion, "the operation of tapping the pericardium has not been largely adopted" (p. 163) is all that is said; no mention is made of paracentesis as practised by Aran, as also by Sibson and numerous others, successfully. From the very nature of the operation, its difficulties and dangers, and the responsibility attached to its performance, it is not very likely "to be largely adopted;" still, in many cases, it is of extreme

value, not only in relieving most urgent distress, but also in saving many a life that otherwise would inevitably be lost.

The preliminary observations on the nourishment of children are well worthy of perusal and evidence much practical knowledge, though it is a question whether "sickly and rachitic children require to be kept longer at the breast than those which are healthy" (p. 196). It frequently happens that the children are sickly or rachitic *because* the maternal milk is not sufficiently nourishing, and a change to good healthy cow's milk often works wonders.

The remarks on cleft palate will scarcely meet with the approval of the rising surgeons of the present generation. The translator even appends a foot-note, entering a protest against the author's conclusions. Diphtheria is evidently a subject the author has seen much of; he writes *currente calamo.*

Lime-water, either as inhalation by means of a spray-producer, or locally applied, is, according to the author's experience, the best remedy for cutting short the exudation and the necrobiosis, and preventing the absorption of the poisonous matter.

The recent controversy in the medical journals respecting diphtheria and croup seems unnecessary, if we can rely on the differential diagnosis given by the author:—"True croupous exudation is free and membranous, whilst the diphtheritic is parenchymatous with necrobiosis."

Gangrenous stomatitis, described by the author as norma, seems to be more frequent abroad than at home, if we may judge from the fact of no less than 102 cases having been observed, only four of which recovered. Stomatomykosis is the name suggested for the ordinary parasitic aphthæ affecting the mouth of infants; lime-water and borax are the remedies relied upon. The remarks on dentition and its dangers are practical. No mention is made of the employment of chloral, which in many of the distressing nervous symptoms due to the irruption of the teeth is invaluable, and far to be preferred to any preparations of opium. In treating of invagination, or intussusception, "gastrotomy as a last resource may be entertained;" but distension of the bowel by air or fluids with the prospect of relieving the obstruction is not referred to—rather an important omission, judged by the standard of recent experience.

In speaking of parenchymatous nephritis, the author says, "The presence of *hyaline* casts must be proved ere the diagnosis can be made with certainty (p. 276.) The importance of producing diaphoresis by means of vapour baths is not insisted upon with sufficient force. The employment of chalybeates in the later stage of convalescence is not even alluded to.

The author has evidently not seen the cases published by Mr. John Wood of successful operation for the cure of ectopia vesicæ,

or he would not assert that "a radical cure of this deformity is hardly possible" (p. 283).

We are glad to find Dr. Steiner has not shirked the duty of alluding to a very distressing practice indulged in by even very young children of both sexes, viz. masturbation. This is far more frequent than many have any idea of, and exerts an influence upon the growing child very prejudicial to its future well-being, intellectually, morally and physically.

"The use of sewing-machines, which has become of late so prevalent, has an evil influence in this direction," he thinks, "especially for young girls; and he has heard several times from women expressions of opinion in this direction." The employment of blistering, repeated from time to time, one of the most efficacious means of dealing with this habit, is not sufficiently insisted on.

The subject here briefly touched upon, although one of vital importance, is generally remitted to the domain of neutral territory. In no work that we are familiar with on children's diseases, Dr. Steiner's excepted, is the question discussed; the general treatises on medicine seldom even allude to it, and only when the final stage, dementia, is attained do we find it figuring in the reports of our asylums as a probable cause of mental defect. It is doubtless a difficult question to handle, but that is no reason for deterring us from making the attempt to rescue it from the hands of quacks and unprincipled adventurers who trade upon the baser passions of our nature.

Parents have no suspicion in many instances why a child is so listless and apathetic, so disinclined for exertion and so easily tired; the medical man is consulted, but from the difficulty of getting any facts and from a wish to avoid injuring the feelings of the parents, few inquiries are made; and whatever may be his suspicions, nothing is said, a tonic is prescribed, and the boy drifts on into confirmed onanism, becoming weak and vacillating, an easy prey to the advertising quacks.

Under the heading, General Diseases of Nutrition, a full description is given of rickets, scrofula, tuberculosis, purpura, and rheumatism.

Syphilis, which by nearly every authority is grouped among the general or diathetic diseases, is here classed with the zymotic diseases, along with the exanthemata, cholera, &c. Dr. Steiner affirms that rickety children often have a precocious mental development, and are restless, irritable, and easily excited (p. 309). Respecting the latter clause, doubtless, it is correct; but, as a general rule, the rickety child is far below the average in mental capacity, though there may be no symptoms of idiocy.

The author refers to the influence of syphilis, struma, unhealthy occupations, and sexual excesses in the parents, as forming a very

important cause in the production of rickets among children, more especially when the mother has been subject to their influence.

The method adopted in speaking of scrofula will approve itself to the student, the subject being treated of seriatim under the different organs affected, as lymphatic glands, skin, mucous membrane, bones, joints, &c.

The relation of tubercle to scrofula he regards as still far from being definitely determined. If not identical, they are diseases which are very closely related in their causes, in their pathology, and in their clinical history; and, though they often occur coincidently or in sequence, it is not a matter of necessity that they should do so.

A combination of the tubercular and syphilitic diatheses in the parents he regards as strongly favouring a tendency to scrofula, and the too early employment of starchy food, or too great excess of it in the dietary, is doubtless sufficient to explain a large number of cases.

The treatment suggested, both general and local, is clear and comprehensive. "For the affections of bones and joints some special surgical treatise had better be consulted" (p. 325).

The most important factor in the production of tuberculosis the author regards as hereditary tendency; but the disease may be developed independently by bad sanitary conditions, and also by the embolic impaction (Einschwemmung) of purulent and cheesy matter. The various theories with regard to this interesting subject of Buhl, Virchow, Waldenburg, Cohnheim, Frankel, Klebs, Villemin, Niemeyer, and Schüppel, are briefly referred to.

The bronchial glands were found to be affected in 275, the lungs in 175 only of the cases observed by the author. The main points in the treatment are briefly glanced at. "Cod-liver oil, when it can be borne, and the whey treatment, are our most efficient methods of cure, but unfortunately they very seldom effect it" (p. 330).

Purpura is classed among the general diseases of nutrition, and very rightly so, for we generally find a history of "insufficient and improper food, together with residence in cold, damp, and badly ventilated dwellings," which unquestionably exert a considerable influence in the production of purpura, though it may be also secondary to some other disease, as the author points out. A special form of this dyscrasia, indicated by Schönlein—peliosis rheumatica—is here referred to. The symptoms are lassitude and pain in the joints, these latter being swollen and very painful, especially on being moved, and they present all the appearance of joints affected by acute articular rheumatism. These symptoms last from two to four days, and are then followed by the appearance of a petechial eruption.

The section on zymotic diseases is clearly and concisely written,

though we miss many useful suggestions as regards the treatment of complications. In scarlatina, *e.g.* "if uræmic symptoms appear, quinine in large doses and cold packing will be found of service." No reference is made to vapour baths, diaphoretics, or cathartics. Quinine, digitalis, and the mineral acids are the remedies chiefly relied upon in reducing the temperature in the exanthemata.

Of 420 children suffering from smallpox, 315 had not been vaccinated, and 208 died, or 66 per cent.; whilst of the 105 vaccinated only 14 died, or 13 per cent. Of 12,000 vaccinations performed in the Prague Hospital no case of communication of other diseases by the agency of the vaccine virus is known. These statistics will doubtless be interesting to the anti-vaccinationists.

The section devoted to diseases of the skin is scarcely so comprehensive as we should have expected, though the more important and most frequent of these diseases are mentioned, and the general indications for treatment given. The author puts forth a fact worthy of notice in speaking of eczema. More than a thousand cases having come under his observation, local treatment, without respect to their duration or extent, was almost always resorted to, and he is not aware of any fatal case having occurred during or after such treatment. He does not agree with those who recommend abstention from local treatment, lest the curing of the rash induce mischief of a more serious kind, as meningitis, hydrocephalus, &c.

A few words as to the manner in which the translator has accomplished his task are all that is necessary. On comparison with the German the translator will be found to be as literal as the different construction of the two languages would admit of; the sense or meaning of the original being invariably given, even when some transposition was necessary to suit the English idiom.

"All thermometric observations have been rendered in the centigrade scale, and all measurements in centi- and millimètres." This will possibly prove a little confusing at present to the English reader, who will be at a loss to understand how long a child who measures forty-nine centimètres really is, no table showing the relation between the two measurements being given—a decided want. Again, "the weight of the body of a new-born child averages from 3 to 4000 grammes" (p. 2) is a statement that will puzzle many.

It would have been well also to have anglicised some of the expressions employed, *e.g.* if retraction of one lower extremity occur, "then gonitis, coxitis, or psoitis may be suspected" (p. 3).

In all material points, however, the English version loses nothing from its translation, and gains much by a few judicious interpolations.

In many respects the book is one that the student and junior practitioner will be glad to refer to, but more as a classical than as a clinical memoir. We miss the details regarding treatment that we are familiar with in West and Tanner. The indications are too

vague to satisfy the wants of most students or young practitioners. There is no appendix of formulæ, and very few instructions as to what combination of drugs or what doses are requisite. We have given a sufficient account to enable the reader to form a fair idea of the value of the work. It is printed in a clear readable type, and in every respect does credit to its well-known publishers.

Materia Medica and Therapeutics—Vegetable Kingdom.[1]—The work which now lies before us is one with which it is not easy to deal. It contains much which is new and has been highly commended, but the greater part of the new material is of the most questionable character, whilst we make bold to say that the majority of those who have commended the work have hardly been in a position to do so from a thorough knowledge of the subject. The newer matter, indeed, is almost wholly taken from two sources, the later German researches and homœopathic literature. The opportunity of bringing much interesting matter from the German before the British profession depends in great measure on the remissness of former writers in the same field; even the author himself seems ignorant that very much of the material available in the Brothers Husemann Pflanzen-Stofle, which he uses so extensively, is to be found in a work which he does not refer to once, viz. the same brothers' edition of Van Hassetl's toxicology, now nearly twenty years before the world.

As to the rest of this new matter, it is neither more nor less than pure homœopathy preached in the ordinary bungling homœopathic manner, and this we are prepared to show. The source of this knowledge is not far to seek. Dr. Phillips was long known as a prominent homœopathic practitioner, but by degrees he became more and more separated from the homœopaths, until at last he was formally reconciled to old physic by being admitted a member of the Clinical Society. Such being the case, there are good grounds for animadversion on the part of homœopaths, who most justly say, here is a man preaching pure homœopathy, and yet his teachings are accepted with something approaching to admiration by the body of the profession. We confess we here hold with the complainants, for this is certain; either Dr. Phillips's teaching must be rejected, or homœopathy and old physic become one and the same; the only distinction of any importance left is the dose, in which again the two opposing bodies are rapidly converging. But there is one characteristic of the educated men of our section of the profession, a characteristic which, we trust, will ever steadily continue, and this assuredly tends to separate them from ordinary homœopaths. To us is given a pathology which we seek after more and more as a basis

[1] *Materia Medica and Therapeutics—Vegetable Kingdom.* By Charles D. F. Phillips, M.D., F.R.C.S.E. London. Pp. 584.

whereupon to work; to the homœopath but a bundle of symptoms; and this we hope to make abundantly apparent from the present treatise.

These charges, therefore, we make bold to bring against Dr. Phillips's work, that it displays a profound contempt for ordinary pathology, and that it teaches the system of treating a malady by symptoms alone. We put it therefore to the public if this is a book to be received as a recognised authority on the treatment of disease? Let us turn for the proof of our assertions to the book itself.

Dr. Phillips's work on Materia Medica and Therapeutics as far as yet published deals only with the vegetable kingdom; the other articles of the materia medica are left to be dealt with hereafter. He classifies the various agents in their botanical orders, but groups the orders somewhat differently from the system usually adopted. In speaking of a plant, first are given its botanical descriptions; secondly, and in a certain fashion, its chemical characteristics; thirdly, its physiological effects; fourthly, and as a rule more fully, its therapeutical effects; and fifthly, its pharmacopœial preparations are briefly disposed of. What can have induced the author to adopt such a scheme we know not; we can understand a regular treatise dealing with the botany of the plant under consideration, but in such a treatise as the present a section on the subject, especially such as the sections here given, are but repertories of useless lore, giving bulk to a book which might have been much smaller and certainly none the worse for much excision.

The first group of plants discussed is the Ranunculaceæ, comprehending in it, however, several plants not unusually employed in ordinary medicine. The first and the most important plant discussed is—Aconite. This powerful drug has long been in the hands of homœopaths, whilst foolishly rejected by many regular practitioners, and consequently we might hope to learn something from the notice here given, but there is not much. In the first place, the account given of its physiological action is meagre in the extreme, and is mainly quoted from authors of little authority nowadays. As to its therapeutical value it is now generally conceded that in the early and acute stages of many maladies of the so-called inflammatory kind aconite does good if anywhere, but this is not everything. Thus it does good in pneumonia, but only in the early stage; it does not affect condensation, and it is only fair to mention the fact that Dr. Phillips recognises their tendency to spontaneous defervescence at certain periods in pneumonia which render all statistics connected with the disease more or less fallible. Our experience is that in some cases it may do good, but is altogether unreliable. So of acute rheumatism, this drug has been frequently used in that disease and as frequently lauded or despised. Dr. Phillips says he has never known heart disease occur if aconite was given from the beginning;

the same may be said of many other drugs, the truth being that the great risk of heart mischief is in the beginning; or again, should the disease recandesce, not in the ordinary course of the disease. Our own experience is that it is of little or no use; nor do the objections raised by the author apply to such experience as regards the inefficiency of the drug, for the aconite was given in small and frequent doses, and was active enough when used for other purposes.

In some places Dr. Phillips' homœopathic training crops out unpleasantly, especially as regards symptomatology. Thus we find under aconite a paragraph as follows:—"Palpitation: aconite is also of great use in those cases of palpitation of the heart which depend upon simple hypertrophy of the left ventricle. On the other hand, in hypertrophy of the left side of the heart, with diseased valves admitting of regurgitation, aconite is dangerous."

Is there such a thing as simple hypertrophy of the left heart? if so, what is it due to? The only form with which we are familiarly acquainted is associated with contracted kidney, and is therefore not simple, or to contracted aortic opening, which is still less so. Mark, again, the signs and effects of *regurgitation* by the two orifices are different. This is homœopathic pathology.

With pulsatilla begins a small group of substances, including bryonia and actæa, almost exclusively used by homœopaths; consequently here we find the information is almost purely from homœopathic sources; their authority cannot be quoted, though their statements are used, and hence the astounding statements stand wholly on Dr. Phillips' responsibility. This authority may be good or bad, but we question if ophthalmic surgeons would care to trust to pulsatilla internally and externally in purulent ophthalmia of children, or even to gonorrhœal ophthalmia, as we are here advised.

We have accused Dr. Phillips of the direct teaching of homœopathy after their own fashion of symptoms only. We may select this as a sample.

Dyspepsia.—Pulsatilla is a good medicine in many of those cases of dyspepsia or of subacute gastritis met with in phlegmatic temperaments, where we find some or all of the following symptoms present, namely, depression of the nervous system with fear of death; loss of appetite; white and thickly coated tongue; little or no taste, or if taste be present a sensation in the palate of greasiness; sensation of mucus about the mouth and gums; nausea with an inclination or wish to vomit; flatulency; heartburn; occasional pains and flatulent colic in the epigastrium; sick headache; dry cough; coldness and clamminess of the extremities and often likewise of the entire surface of the body, generally accompanied by constipation or by diarrhœa. When the diarrhœa is attended by mucous discharges or by active piles, the pulsatilla quickly removes them. Now, what

means this farrago of symptoms? where do we find anything like it save in homœopathic works? But the truth is that in the whole of this section the authorities are homœopaths or eclectics, but as their names cannot be taken from Husemann they are silently passed by. There is no necessity for any one who understands homœopathy to go beyond the various articles included under the natural order Ranunculacæ, especially pulsatilla, hydrastris and actæa, to be convinced of the character and the sources of the work.

If we go further on through the book the same unsatisfactory features are notable; there are few indeed of the articles which could pass without question. As, however, there is a certain recognised basis to go upon, the information is not in most cases wholly homœopathic. When, however, we arrive at such articles as cocculus indicus, sanguinaria, bryonia, arnica, &c., we cannot fail to be struck with the characters of the sections; they sound almost like hydrastis or actæa over again; and, like most homœopathic literature, amount to neither more nor less than sheer nonsense. It is no part of our purpose to be unfair to the book; we are quite free to admit that there are some good articles in it. But there is far too much trash in it; its materials have not, as a rule, been sought from original sources; there is an air of learning about it which is hollow; men of all values are quoted save the author's old homœopathic friends, and poor authorities often take the place of good ones; moreover, the book is cumbered with a good deal of useless rubbish. It is quite certain that this last statement is true of most books of materia medica and therapeutics, but here a considerable proportion of this kind of stuff is voluntarily introduced. In short, were the book cleared of superfluities, it might be reduced to a much smaller size and a less pretentious character. Our main purpose in writing this notice of the book was and is to direct attention to its dangerous character as a work of unreliable authority, and we conceive that what we have already said and quoted will suffice for this purpose. Were it necessary we could multiply these illustrations indefinitely, but we think we have done enough. We are quite unacquainted with the author save by his book, and our observations have been made in no hostile spirit to himself; but we conceive it to be our duty to make known to the public the character of certain passages in the work which essentially mar the whole.

Wanklyn's Tea, Coffee, and Cocoa.[1]—Had a little more time and labour been bestowed upon the preparation of this pamphlet it would have proved serviceable to many public analysts. The author acknowledges in his preface that he has done little more than collect

[1] *A Practical Treatise on the Analysis of Tea, Coffee, and Cocoa.* By J. A. Wanklyn. Pp. viii and 59. London, 1874.

the material which had been accumulated by the numerous chemists who have examined tea, coffee, and cocoa. His own additions to our knowledge are very small, and from imperfect acquaintance with the chemical literature of food analysis, he too often claims, as original, methods and facts long previously known. Witness his announcement as a new process of the taking of five grams of milk for determining the residue left on its evaporation—the very quantity recommended by M. Doyère so long ago as 1851. His discovery of manganese in beech leaves, given to the public last year as a novelty, had been similarly anticipated by many years.

A good manual of food analysis is really much needed. It would not be a bulky volume even were it to include all the necessary processes for the examination, not only of tea, cocoa, coffee, milk, and other articles of food in daily use, but of water also. At present, when any kind of material used as food has not been the subject of frequent analysis, the chemist to whom it happens to be sent may never have submitted it to examination before. He is probably ignorant, not only of the standard of average composition with which the sample if genuine should agree, but of the tests, qualitative and quantitative, which are best adapted to reveal its character. Such information as exists in reference to the chemistry of the material under examination is not only scattered in isolated papers contained in scientific journals, chiefly foreign, but it requires adaptation and further work before it can be made to answer the end in view. Some adequate steps should be taken at once to supply the deficiency here pointed out. Surely the Chemical Society, the British Association, and the Society of Public Analysts might between them decide upon a plan for providing a really standard book on food analysis. A committee should be appointed to whose several members might be assigned the duty of writing the different chapters of the proposed volume, a responsible editor revising the whole work.

Watts' Dictionary of Chemistry, 2nd Supplement.[1]—Nothing is more difficult to review than a dictionary. We have little to say concerning the present volume, having already given our estimate of the great value of the five volumes of the dictionary itself and of the first supplement. Two only of the contributors to the preceding volumes have written in this second supplement, and their articles are not purely chemical, relating as they do to magnetism and light. Three new contributors were, however, secured by Mr. Watts, namely, Dr. Armstrong, of the London Institution; Dr. Newell Martin, Lecturer of Christ's College, Cambridge; and Mr. R. Warington, formerly assistant to Professor Church at the Agricultural College, Cirencester.

[1] *A Dictionary of Chemistry.* By Henry Watts. Second Supplement. London, 1875.

The first of these three chemists has written a couple of articles, one being on isomeric phenols, and the other on the chlorides of sulphur. Dr. Newell Martin has contributed some seven pages on subjects connected with the physiological chemistry of man; these articles are clearly written, but they give a very inadequate idea of the vast amount of good work which has been lately accomplished in this most important department of inquiry. Mr. Warington's contributions relate to agricultural chemistry, and seem to have been compiled with great care; they take up such subjects as malt, fodder, manure, and root-crops. In the paragraph on malt we notice, however, a serious error, for which it is difficult to account. Malt-dust, which is removed by screening germinated and dried barley, is said to consist of the radicle *and plumule* of the grain. So far from this ever being the case, the quality of malt may always be judged of to a certain extent by the length of the plumule, which remains firmly attached to the finished malt, and, so far from being removed or even removable, can only be seen by raising the coverings of the grain. But such mistakes as this are quite exceptional, so that we may fairly regard these papers on agricultural chemistry as fairly supplementing in some degree the deficiencies of the original dictionary in this particular.

Of the 1215 pages which the present supplement contains, by far the larger number are the work of the indefatigable editor, Mr. Watts. He has executed his task of collecting and condensing the results of recent chemical research (to the end of 1872 mainly) with skill and success. Now and then, of course, some new announcement of an old fact or an old process is given as if it were really a new discovery worthy of record as such. This is almost the only kind of defect in the present volume of which we feel inclined to complain. Referring, for example, to the article on milk (pp. 811—813), we find nothing of that which is new in it worth preserving, while the remaining statements in it, if announced as new discoveries, have in reality been familiar to some chemists for years. The well-known plan of testing milk by taking the specific gravity, not of the milk itself, but of the whey or serum which it yields on artificial curdling, is cited on the authority of a recent writer in the 'Chemical News.' We notice the same tendency to give as new many facts which are not merely old, but have acquired an almost antiquarian interest, in other works besides that under review. Such a tendency is common in compilers of popular scientific works, but it is disappointing to meet with it in the otherwise invaluable abstracts published in the 'Monthly Journal of the Chemical Society.' These abstracts have been largely drawn upon by Mr. Watts, and it is probably in this way that the hypertrophy of the present book may, in some measure, be explained.

Forces which carry on the Circulation of the Blood.[1]—In this little brochure Dr. Buchanan seeks to demonstrate the influence the pneumatic forces exert in aiding the heart to maintain the circulation of the blood. Most physiologists, trusting the reasoning advanced by Dr. Arnott, consider these forces, if they have influence at all, to be quite subsidiary to the cardiac contraction, but Dr. Buchanan by a very logically maintained argument establishes their importance, and suggests various experiments by which it may be proved.

The course of his argument renders it necessary that the actual force exerted by the heart should in the first instance be determined, and he proceeds to point out the cause of the strikingly discrepant results obtained by Borelli, Hales, and Kiel, viz. that these observers proposed different problems to themselves, the first seeking to determine the absolute force of the heart; the second, the absolute force of the fibres of the left ventricle as combined and operating in the mechanism of the heart; and the third, the effective force of the heart in carrying on the circulation of the blood, or in other words, the work done by the heart. Dr. Buchanan takes up the last problem, and shows that, to calculate the effective force of the heart, it is necessary to know the mass of blood to be moved, the velocity with which it moves, and the obstacles which oppose its progress. In pursuing this inquiry he estimates the number of contractions of the heart per minute at 72, the primary sectional area of the blood-vessels at ·4187 of a square inch, and the quantity of blood discharged at each stroke two ounces, propelled into a tube already full of blood, and having a height of eighty-seven inches; such a column weighs twenty-two ounces. He further estimates that the velocity of the blood issuing from the heart is ten inches per second, which is certainly below the truth; "the heart, therefore, at each contraction, exerts a force which would be in equilibrium if counterbalanced by a weight of 22 oz. + 129 grs.; and that the mode in which this force is expended is most easily understood by supposing that we have a tube 87 inches in height, and ·4187 of an inch in base, that this tube is exactly filled with blood, and that at each contraction of the heart two additional ounces of blood are forced into it at the lower end, lifting the whole column over a space of 8 inches, and causing an equal overflow at the top. This represents accurately the labour of the human heart, and supplies us with two numbers to express it: the one, 22 oz., being the weight of the column of blood, and the other, 8 inches, the space over which the column is lifted. The former of these numbers denotes the resistance that has to be overcome in forcing 2 oz. of blood into the aorta

[1] *The Forces which carry on the Circulation of the Blood.* By Andrew Buchanan, M.D., Professor of Physiology in the University of Glasgow. Second edition. London, 1874.

and pushing before it the whole mass of blood in the blood-vessels; the latter, again, denotes the velocity with which the blood issues from the heart. Multiplying those two numbers together we obtain the momentum which the heart communicates to the blood, 220 oz. moving with a velocity of 8 inches during the period of a pulsation, or of 10 inches per second, or 50 feet per minute. This is equivalent to 176 oz. (22×8) lifted one inch, or 14·66 oz. lifted one foot, during the period of a pulsation, or of 65·9 foot-pounds in a minute, or 42·3 foot-tons in twenty-four hours." Dr. Buchanan then shows the errors that underlie Hales' application of the hydrostatic paradox to the action of the heart, and of his system of measurement of its internal superficies.

These are essentially, he thinks, that the heart differs from the experimental demonstration of the hydrostatic paradox in having an aperture of ·4187-inch diameter which permits the escape of fluid as soon as the heart begins to contract, as indeed is demonstrated by the fact that the hæmostatic column only rises to its full height at the commencement of the contraction of the ventricles, and then begins immediately to subside, so that it is obvious that the blood escaping along the aorta must gradually diminish the tension on the walls of the ventricle. The second fallacy in Hales' argument is, that whatever may be the force exerted by muscular fibre at the commencement of contraction, it gradually diminishes as contraction proceeds. The correction requisite for these two errors in calculation Dr. Buchanan considers to be that the mean force of the muscular fibres as arranged in the human heart is no more than one eighth of their original force, whilst the mean size of the ventricular surface must be reduced to one fourth. "Combining these two ratios we have a total medium estimate of one thirty-second. Now if we multiply," he goes on to say, "22 oz. by 32 oz., the result is 44 lbs., which would be very nearly the initiatory power of the heart if we assume the size of the ventricular surface as 13·3984 square inches. The conclusion, therefore, is that there is no difference between the effective force of the heart estimated at 22 oz. and the mean absolute force = $\frac{44}{32}$ lb. or 704 oz. divided by 32 = 22 oz."

Having thus determined the absolute effective force of the heart, Dr. Buchanan proceeds to consider what other forces aid in carrying on the circulation of the blood. These are the muscular contractility of the blood-vessels, which is very small; and secondly, a central pneumatic force or the atmospheric pressure towards the chest and heart rendered effective by a central dilative force. This last he considers to be much more effective than is generally admitted, and he opposes Dr. Arnott's objection that any aspirative action can be exerted on thin-walled vessels like the veins on account of their tendency to collapse, by pointing out that the pressure of the blood would keep them constantly full. He criticises

with much force Dr. Arnott's "bloodless experiment" intended to disprove the suction action, in which a syringe terminates in two nozzles, one dipping into water whilst the other preserves free communication with air. In this experiment, as might be expected, as long as the air enters freely, the water rises to a very small height only. Dr. Buchanan modifies this typical respiratory apparatus by introducing a sac into the syringe with an exterior opening through which the air rushes and distends the sac on raising the piston. This, it must be acknowledged, fairly represents the action of the diaphragm and chest in respiration, and proves that a considerable force is in constant operation.

Dr. Buchanan considers that asphyxia is the experimentum crucis of the doctrine of the pneumatic agency of the chest. When the act of breathing ceases the heart is deprived of all assistance from the pneumatic force of the chest; it has to contend single-handed against all the resistances which oppose the onward movement of the blood—a task for which its utmost efforts are ineffectual; it labours and palpitates in vain; the blood accumulates in the capillary vessels of the system and more effectually oppresses the brain, which it does the more readily that it is now no longer as oxygenated blood, but as black or venous blood, that it stagnates in the cerebral vessels; loss of consciousness of sensibility and of all other forms of nervous energy speedily ensue, and these are the forerunners of the extinction of life.

The phenomena of the fœtal circulation are impressed by Dr. Buchanan as supporting this conclusion; and he maintains that the foramen ovale is simply a means of allowing the right heart to aid the left in carrying on the circulation. Immediately after birth the pneumatic forces come into play and the foramen ovale closes up. The pneumatic forces are consequently about equivalent to the force exerted by the right heart.

The phenomena resulting from the opening of a vein even at some distance from the heart lend considerable support to Dr. Buchanan's explanation; still he does not appear to us quite to meet the difficulty experienced by Poissenille of obtaining evidence of a suction power in veins more remotely situated, and he seems also to overlook the very decided reflux that may be observed to occur during expiration in the great veins of the neck, and which is opposed to his view, that no fluid escapes from the chest at this period. The subject is one that possesses much interest from a practical as well as a theoretical point of view, and Dr. Buchanan deserves great credit for his attempts to elucidate a confessedly difficult bit of physiology.

On Tuberculous Arthritis.[1]—Dr. Roux professes himself to be a

[1] *De l'Arthrite Tuberculeuse, Démonstration de l'Existence de cette Affection par Inoculation de produits synoviaux. Étude accompagnée d'Observations re-*

believer in the inoculation of tubercle, although he does not bring forward any very strong arguments or facts in support of this view. The practical part of his work seems to aim at distinguishing the different kinds of tubercular arthritis, and he divides them into two categories—one in which a pulmonary lesion is anterior to the articular, and another in which the articular disease is first developed and precedes the visceral affection by several months. The cases falling within the second division are much more frequent than the others, and Dr. Roux refers to the numerous instances where patients suffer for several months or even years with white swelling, and are then attacked with intercurrent phthisis, which sometimes becomes the predominant affection, sometimes is only obscurely developed, and sometimes is discovered only after death. With regard to treatment, he advocates the use of the knife as being the best means of arresting the disease when the lungs are not yet positively affected, and he gives two cases in illustration, in one of which the leg was amputated for a white swelling of the knee, and in the other a resection of the elbow was performed for tubercular disease of that joint; in both these cases complete recovery took place, although there had been previously threatenings of pulmonary mischief. The question of amputation or resection is regarded somewhat differently in England and in France, for whereas in the former country, according to Dr. Roux, resection is performed on all kinds of joints and sometimes for comparatively slight affections, in France, on the other hand, resection is never performed on the lower limbs, except sometimes on the foot. The reasons given for the abandonment by the French surgeons of resection of the knee and the hip are stated by Dr. Roux to be the great mortality attending the operation, and the want of sufficient repairing power in the joint, and the difficulty of procuring perfect anchylosis.

Stille's Therapeutics and Materia Medica.[1]—It is unnecessary to do much more than to announce the appearance of the fourth edition of this well-known and excellent work. We are informed in the preface, however, that for two years it has been out of print, and that the unavoidable delay in preparing the new edition has given opportunities to the author to revise the whole work and to add

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cueillies à l'Hôtel-Dieu de Lyon. Par le Dr. J. Roux, Ancien Interne des Hôpitaux de Lyon.

On Tuberculous Arthritis, a Demonstration of the Existence of this Affection by Inoculation of Synovial Products. A Study, accompanied by Observations collected at the Hôtel-Dieu of Lyons. By Dr. J. Roux, formerly House-Surgeon at the Lyons Hospitals.

[1] *Therapeutics and Materia Medica: a Systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History.* By Alfred Stillé, M.D., Professor of the Theory and Practice of Medicine and of Clinical Medicine in the University of Pennsylvania, &c. Fourth edition, thoroughly revised and enlarged. In two volumes. Philadelphia, 1874.

about 250 pages of new matter. Several new articles have been introduced, the chapter on electricity has been almost entirely re-written by Dr. Mathew J. Grier, and the nomenclature throughout has been made to conform to the last edition of the (United States) 'Pharmacopœia.' The book is arranged according to the therapeutical action of the respective drugs, and Dr. Stillé states that continued study, observation, and reflection have tended to strengthen his conviction that it is a mischievous error to deduce the therapeutical uses of medicines from their physiological action, and that clinical experience alone is the true and safe test of the virtues of medicinal agents.

Commentary on the British Pharmacopœia.[1]—Notwithstanding the great number of books already published in explanation of the drugs and preparations in the British Pharmacopœia, Dr. Walter Smith's volume will, we think, be found a welcome addition to the list. It differs from other class-books on Materia Medica in not reprinting the text of the Pharmacopœia, so that its size is proportionally reduced and needless repetition is avoided. We cannot too strongly urge the necessity of every medical practitioner possessing a copy of the official publication, and, if so, then the commentary need not embody all the descriptions and formulæ over again. Those who only wish to know how to prescribe with accuracy and safety will be satisfied with the 'British Pharmacopœia' itself, but the great majority of practitioners and all students must wish for much more, and works like this before us will supply the deficiency. It is of course quite impossible to enter into detail as to the manner in which Dr. Walter Smith has executed his task, but we must remark that the book is very well and clearly printed, and the descriptions are very good. The new notation in chemistry being now very generally understood, it is adopted in the chemical sections. The natural history of the animal and vegetable substances is briefly described, but details are given of such plants as are indigenous to Great Britain and Ireland or are commonly cultivated in those countries. Another feature in this work is that the recently published 'Additions to the British Pharmacopœia' are incorporated in the text in their alphabetical order, and some brief notices are also given of some non-officinal drugs which are at present most in demand or which promise to become permanent additions to our stock of remedies.

Roy on Burdwar Fever.[2]—The author, a native of India, who

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[1] *Commentary on the British Pharmacopœia.* By Walter G. Smith, M.D. Dublin. Pp. 766. London, 1875.

[2] *Essay on the Causes, Symptoms, and Treatment of Burdwar Fever, or the Epidemic Fever of Lower Bengal.* By G. C. Roy, M.D., F.R.C.S., Surgeon, Bengal Establishment, &c. Calcutta, 1874 (pp. 92).

seems to have studied in this country, is Inspector of Dispensaries in Burdwar. After nearly two years' experience there he embodied his views as to the causes of the fever which has prevailed in that and the neighbouring districts of Lower Bengal for a considerable period in a paper which was submitted for competition for the prize offered by the Viceroy of India for the best essay on that subject. Though not the successful competitor, he has since enlarged the original paper by the addition of much new matter, and it is in this form we have to deal with it.

The cultivation of rice has long been one of the chief occupations in many parts of the delta of the Ganges and Brahmaputra, and the increasing demand for this article of late years, both for home consumption and export, has led to an enlarged area being brought under crop, not only in the delta itself, but in the low alluvial districts to the west of it, the rivers of which rise in the high land of Cheta Nagpore and flow into the Hughly. Rice requires much water while growing, and the cultivators are accustomed to ensure the necessary supply of this by raising small dams round their fields, which retain enough of the rainfall to afford the moisture required for bringing the crop to maturity. A low, flat, alluvial district, subject to the flooding caused by tropical rains, is always subject to fever as the waters subside, but the evil is aggravated to no inconsiderable degree by the conditions under which rice cultivation is carried on, and, accordingly, the older rice districts in the centre and eastern parts of the delta have been long characterised by the frequency and severity of the malarial fevers in them. It was only within the last twenty years, according to our author, that fever became prevalent in some of the western parts of the delta, or, more probably, the unusual prevalence of the disease in these districts has only attracted attention within that period. Be this as it may, however, every few years it has been extending itself westward over fresh country, and by 1869 it had involved Burdwar, Bancoorah, and Midnapoor. The progress of the disease in a village is thus described:

"The first year of the invasion is characterised by an increase of ordinary fever cases, but it subsides completely with the advance of the season; the second year counts more victims and the duration of the disease becomes longer. With the prolonged suffering the complications begin to appear. A little respite is enjoyed in summer by those who are free of complications, but only to suffer again in the ensuing rains. In the third year more mortality takes place from primary attacks and secondary complications, as it finds the system so reduced from constant suffering that it is ill fitted to sustain an assault. Enlargement of the spleen and the liver, anasarca, anæmia, ascites, cancrum oris, now become common sights. In summer, instead of improving, the suffering continues. In the

fourth year a slight abatement of its severity is observed, but fatal cases occur among chronic patients, who succumb under slight causes of exhaustion. In the fifth year improvement is more manifest, and temporary abeyance in summer shows itself. In the sixth year the majority recover, gaining in flesh and strength, but a permanently enlarged spleen is left behind." (P. 46-7.)

Though the fever is clearly malarial, it has been supposed that an element of contagion, having been added in some way to the original disease, causes it to spread from village to village through human agency. The author, however, states he had been able to watch numerous cases of the worst type in individuals, in the early and later parts of the season, who did not impart the disease to their attendants, while, during the height of the rains, it became general and widespread, as is common with fevers depending on endemic causes. As to type, he points out the frequent combination of continued remittent and intermittent fevers in the same case, the one merging into the other in various ways as the disease runs its course, a peculiarity every one who has observed fever closely in malarious localities must have recognised. He also mentions one variety in which, instead of convalescence being established in eight or nine days, the disease goes on and assumes the typhoid type, a combination we believe is not unfrequent when the ordinary cause of enteric fever exists in any locality at the same time with malaria. In his descriptions, too, of a fatal termination from cerebral complications on the fourth or fifth days of the fever, from exhaustion in the second week, whether arising from deficient nourishment or accelerated by too few evacuations, and from pulmonary congestion in the third or fourth week or later, old tropical practitioners will recognise phases of disease they must frequently have encountered, and which, until they became familiar with them, were always sources of serious anxiety and doubt. One complication noticed, cancrum oris, is not common elsewhere in fevers of this description, though sometimes met with; it mostly occurred in boys under fifteen, and "generally towards the end of winter or beginning of summer, when, after prolonged and continuous suffering, vitality is reduced to its lowest ebb."

Various causes have been assigned by different writers for the greater frequency and severity of this fever of late years. Of these the principal are—the gradually increasing imperfection of the drainage consequent on the deposition of alluvial matter in the channels of the delta, the impediment offered to the free escape of surface water by railway works, the numerous pools required for the cultivation of rice, the increase of jungle in the neighbourhood of many villages, and, lastly, bad water for domestic use. Now, as the author points out, all these have been in operation for a very long period, except the interference with the surface drainage caused by

the railway works, which is not material in amount, and applies but to a very limited portion of the districts affected; and though he thinks each has contributed to the development of the fever at various points, yet the disease was by no means so generally diffused as the existence of these conditions would lead us to expect, and it presented a varying intensity, recurring in greater force every few years, and then manifesting a disposition to spread into districts previously comparatively exempt. To explain these apparent anomalies the author observes, every poison requires a favorable soil to become endemic in a country, and it is the addition of a ferment in the air that gives it an increased potency, and thus converts it into an epidemic agency. The ferment, he considers, is generated by the long-continued action of accumulating filth, and it is communicated to contiguous places, spreading devastation from village to village in a series of years. We fear this theory will not prove satisfactory to those who submit it to a critical examination, and compare it with what is known of the manifestations of fever elsewhere in India, or in other countries. That there are many sources of malaria in operation in the districts in which this fever has prevailed there can be no doubt; that fevers have become more frequent in them from time to time under the operation of these causes, which, for want of a specific knowledge of their nature, we have hitherto included under the term "epidemic influence," is equally clear; but the particular cause which has led to the remarkable prevalence and severity of the disease, in a comparatively limited though gradually extending area, has still to be indicated. We hope Dr. Roy will continue to apply the powers of observation of which his essay affords evidence to the investigation of the facts which come under his notice, with the view of eliciting more precise information on this very interesting question.

Hamilton on Syphilitic Osteitis.[1]—Mr. Hamilton's lectures, though not containing anything new, will furnish the student with a clear and terse description of syphilitic bone disease, and with some useful practical hints for diagnosis and treatment.

The subject is treated under three heads—periostitis, the bony node, and the soft node—and is illustrated by the relation of numerous cases.

Although periostitis is spoken of separately, Mr. Hamilton justly remarks that clinically we find that even slight cases of periostitis involve changes in the surface of the bone also. A good description with an illustrative photograph is given of the hard or bony node, which, though most often found among the late manifestations

[1] *Lectures on Syphilitic Osteitis and Periostitis.* By JOHN HAMILTON, Surgeon to the Richmond Hospital in London and Dublin. 1874.

of the disease, is sometimes also seen as an early symptom, accompanied by sore throat and rashes upon the skin.

An interesting case is related wherein a node upon the fifth rib closely simulated a mammary tumour. "Osteitis and periostitis of the spine of the vertebræ are so like spinal irritation and neuralgia that it is only by much care in the examination of the local derangement and the concomitant symptoms with the history that accurate induction is obtained. A still more important and interesting locality is where the bodies of the vertebræ in the pharynx are the seats of osteitis and periostitis. Another situation which is not unusual, and where it might lead to mistake, is the trochanter major of the femur." Cases exhibiting various paralytic symptoms depending upon syphilitic disease of the vertebræ and cranium are given, and also of epilepsy due to thickening of the skull and cerebral membranes.

The most valuable lecture is that on syphilitic disease of the bones of the orbit. The resemblance which these cases bear to malignant disease is well described, and is a matter that the student cannot have impressed too vividly upon his mind. Most surgeons must have seen cases in which protrusion of the eyeball by soft orbital tumours has led to their being condemned as malignant, but which have rapidly recovered under anti-syphilitic remedies.

The soft node is described as "the product of syphilitic inflammation of the bone and periosteum in strumous and broken-down constitutions;" but we think it is by no means confined to such persons. The resolution of these nodes often gives rise to the depressions on the surface of a bone which are valuable evidences of past syphilis. The danger of operative interference with necrosed cranial bones is spoken of, and the treatment by sulphuric acid introduced by Mr. G. Pollock is alluded to.

We think it a pity that Mr. Hamilton should apply to the syphilitic gummata the name of "yellow tubercle," a description which can only add to the confusion already existing with regard to the signification of the word "tubercle;" and we see no reason for separating those cases of periostitis in which the inflammatory products undergo caseous degenerations from the other instances of periosteal nodes.

In the lecture on treatment we are glad to see Mr. Hamilton speaking decidedly upon the necessity for mercury. "There are some members of the profession, I am aware, who believe that not only is mercury objectionable when the bones are diseased, but that their diseased condition is the result of the previous use of mercury. I have sufficiently shown by my recommendation that I do not agree with this opinion. A style of reasoning has been adopted (unsupported by facts) that, when mercury has been previously

given, the diseases of the bones which may afterwards appear are caused by it. On the same principle the deaths from phthisis might be attributed to cod-liver oil, for how few die of that fatal disease who have not previously taken cod-liver oil! I have notes of cases where well-marked nodes, with other secondary symptoms, occurred where no mercury at all had been given for the primary chancre. I have met with many cases of tertiary osteitis and periostitis, particularly of the cranium, where no mercury had been used for primaries or secondaries."

A useful guide as to the effect of mercury is to have the patient periodically weighed. Mercurial inunction is recommended, but we are surprised to see no allusion to the mercurial vapour bath. The great value of iodide of potassium in the later cases is pointed out, and some cases related showing how amenable to treatment many of the syphilitic bone diseases are.

Taylor on Syphilitic Diseases in Children.[1]—This is a careful and elaborate treatise upon a subject to which the author has given much attention. It is based partly upon the observations of others and partly upon a series of cases recorded with great care by Dr. Taylor himself, and may be recommended as an excellent account of a class of cases the nature of which has been often mistaken, and which even now are frequently not recognised as syphilitic. Doubtless Dr. Taylor is correct in saying that many of the syphilitic lesions of the osseous system in infants have been regarded as evidences of rickets or scrofula; and although instances of syphilitic bone disease in infants had been accurately described by Valleix in 1834, and by Ranvier in 1864, no systematic account of the affection had appeared until Dr. Wagner, of Berlin, published in 1870 a paper minutely describing the bone lesions in twelve syphilitic children, and showed that the pathological processes were *sui generis*. These observations were confirmed and added to by Professors Waldeyer and Köbner; and in 1872 an excellent clinical description of the cases was published by M. Parrot in Paris. Dr. Taylor has collected all the published cases, and has added twelve of his own, described with a care that makes them a very valuable record. These are followed by a minute description of the pathology of the affection, and by sections upon diagnosis and treatment. The most common form of osseous lesion in syphilitic infants is an enlargement of the shaft of the long bones, due to an inflammatory affection of the lower part of the shaft, and of the ossifying layer of the epiphysial cartilage, whereby the epiphysis is often separated from the diaphysis. The ends of the bones become thickened externally and

[1] *Syphilitic Lesions of the Osseous System in Infants and Young Children.* By R. W. Taylor, M.D. New York, 1875.

softened within, and may be eventually destroyed by a purulent infiltration leading to destruction of the lamellæ and the formation of cavities. The joint is not usually involved. A similar disease less frequently attacks the short bones, giving rise to nodular swellings upon their surface. Dr. Taylor divides these cases into two classes—"a first in which the morbid processes, as evidenced by swellings, undergo resolution, without perceptible impairment of the structure of the bones, of the parts around and above them, or of the function of the member of which they form a part; and a second in which resolution does not take place, but in which degenerative changes are observed. These degenerative changes may be limited simply to a destruction of the superficial portions of the swellings in greater or less extent, or they may be so severe as to involve the whole diameter of the swelling in destruction or liquefaction, in which event the epiphyses become separated from the diaphyses."

In describing the affection as it occurs in the upper extremity it is pointed out that the swellings are usually, though not invariably, symmetrical, and are more often developed at the internal condyle of the humerus than elsewhere. The ribs are less often affected than the other long bones, and usually only a few ribs are involved. It is necessary to distinguish this disease from the enlargement of the end of the ribs due to rickets. The diagnosis will be assisted by the concomitant symptoms, by the early development of the syphilitic swellings and the absence of a prodromal stage, by the limited number of ribs affected in syphilis, the less symmetrical character of the syphilitic disease, and the state of the fontanelle, the closing of which is not delayed by syphilis. The affection of the other long bones is described with a good deal of needless repetition, and an elaborate description is given of the so-called "dactylitis syphilitica." We do not think this last disease is clearly proved to be syphilitic. It presents several obvious differences from the epiphysial disease described above, and Dr. Taylor admits (p. 149) that in many of these cases of dactylitis "the concatenation of lesions and symptoms which is called scrofula was present," that in many of them there was no history or symptom of syphilis, and that "after careful comparative study of these cases" (syphilitic and non-syphilitic) he must confess "that there are almost no distinguishing points." We think that the character of the swelling, affecting, as it does, the whole phalanx, and not its epiphysis or shaft-end merely, points to a scrofulous rather than to a syphilitic origin.

The cranial bones of children who inherit syphilis sometimes present nodes very much resembling those so commonly seen in the acquired disease. These are among the less frequent of the bone affections of syphilitic children, although, we think, not so rarely seen as Dr. Taylor supposes. The frontal bone is the most often

attacked. These nodes are prone to undergo rapid degeneration, and to lead to the formation of extensive ulcers, and even when resolution takes place depressions are often left in the surface of the affected bone. It is to be remembered also that the inner surface of the cranial bones has been found diseased in syphilitic infants. With regard to the other irregular bones, such as the scapula or ilium, the chief point to be noticed is that the syphilitic swellings are chiefly found "wherever the body of a bone is continuous with an epiphysis."

These bone affections are usually developed within a period averaging between two and six weeks; a number of bones are usually affected at the same time, and the swellings are often symmetrically distributed. The integument is not usually involved, though in severe cases it may become secondarily inflamed, and the joints most often escape. There is not generally much pain, except upon movement of the affected limb; motion is, however, much restricted, so that the limbs often appears paralysed, a peculiarity which has led to the name "pseudo-paralysis" being applied to the disease. The local changes are unattended by any marked fever. It is clearly proved that the bones may be affected by syphilis during intra-uterine life, and when the disease is developed after birth the symptoms generally show themselves during the first six weeks of infancy.

A very important fact insisted on by Dr. Taylor is, that the changes we have now referred to may occur in *acquired* infantile syphilis. This is proved by Dr. Taylor's eighth case, in which a previously healthy child acquired a syphilitic ulcer on the lip from an infant with inherited syphilis. The patient subsequently suffered from well-marked syphilitic symptoms, including the characteristic swellings upon several of the long bones. The author calls attention to the activity and extent of the morphological changes going on in the osseous system of growing children, and points out that the pathological changes due to syphilis may be engrafted upon the physiological processes during any part of the period of development. Dr. Taylor also alludes to the similarity of many of the symptoms of acquired and inherited syphilis as exemplified in several of his cases, and agrees with Mr. Hutchinson that periostitis is a frequent manifestation of inherited syphilis. The treatment advocated by Dr. Taylor is the administration by the stomach of bichloride of mercury and iodide of potassium, and he justly condemns the hypodermic injection of these remedies. We do not agree, however, with his objections to mercurial inunction, which we have often seen act as admirably in inherited as in acquired syphilis, and we may add that we have seen epiphysial swellings disappear rapidly and completely under the influence of grey powder without the addition of iodide of potassium.

Carter on Leprosy and Elephantiasis.[1]—The issue of this handsome volume reflects much credit on the Indian Office. An opportunity has been afforded to Dr. Carter to bring together an immense amount of information respecting the disease he has so patiently and carefully studied. The volume indeed is, as the author states, mainly a compilation. The chief element of it is to be found in the various papers and reports which in a series of years have been produced by the author himself, whether as the result of what he has seen in India or in Norway. A considerable portion is also devoted to the labours of Norwegian observers, which are brought very prominently forward, and the notes contain much of what has been written in India and other parts of the world of late years on this very complicated malady. We have recently devoted so much of our space to the consideration of this disease (see April, July, and October numbers of 1874) that we shall for the present defer any minute examination of its contents. It is probable that the results of the inquiry concerning skin diseases in India which was suggested to Government by Drs. Tilbury Fox and Farquhar will ere long be given to the world; indeed we find that the portion which relates to Medus has already been printed in the East. The Indian Government is commencing a fresh inquiry into the nature and diffusion of the disease. We also expect soon to see what impression his more recent investigations on his way back to Bombay have made on Dr. Carter. Possibly also the Colonial Office may elicit some fresh information respecting the Australian colonies and some of the South Sea islands; from all these sources fresh material may be expected which will help us to a fuller consideration of the subject at a later period.

Meantime we would say that the chief novelty in this volume is the prominence given to febrile symptoms as one of the prodromata of the disease. And to the various forms of cutaneous affections which may accompany it, or are thought to be characteristic of it, of which our author has been able to gain some illustrations from his own sketches as well as from those of Danielson's great work. But perhaps chief among the novelties of the work are the views respecting the essential nature of leprosy which Dr. Carter appears to be gradually adopting. He now regards it less as a dyscrasy, as it has been most frequently considered to be, and attributes to it, like his Norwegian friends, a chronic-infection character.

He thus expresses himself:—"As far as the order and course of phenomena and their probable cause are concerned, the hypothesis

[1] *On Leprosy and Elephantiasis.* With Plates. By H. VANDYKE CARTER, M.D. Lond., H.M. Indian Medical Service. Published under sanction of the Secretary of State for India. London, 1874. 4to, pp. 246.

of a nature in leprosy like to that of scrofula, syphilis, and I had almost added pyæmia, smallpox, &c., seems to offer by far the best clue to their comprehension. Perhaps the vehicle of infection is essentially of the same order. Meantime I am aware of the consequence of these admissions; the hereditary nature of leprosy must be limited to an innate proclivity or predisposition to disease." He almost unreservedly adopts Hansen's theory that "the leprous disease is essentially an implanted one, and derived from without; its structural elements are first located in the skin and adjoining surfaces, and afterwards reach the blood or system. Here, in fact, is a specific malady which, though of chronic character, is yet of parasitic nature, and it is almost a necessary corollary that it should be of infectious nature."

It is not surprising that, thus following in the wake of Hansen, Dr. Carter now is inclined to admit the contagion of leprosy, and also to attribute a much less important share in the production of disease to hereditary transmissions than he himself and most observers formerly did. He practically returns to the old received opinion, that leprosy, like phthisis, was infectious.

The great question to which all this points is this, whether leprosy should again be treated, as of old, as a highly contagious disease. Is the treatment in the Sandwich Islands or in Crete by entire isolation necessary or justifiable, especially if the main reason assigned for isolation, the propagation of the disease by heredity, is now considered doubtful? Is there, or is there not, risk in treating leprous patients in the same hospital with other invalids?

There are two particular facts of which surely it would be possible to ascertain the reality or otherwise. Is it an undoubted fact that leprosy was introduced into Honolulu by the Chinese, and that in the course of about fifteen years the cases of leprosy have increased from 60 to 800 in a population of only 40,000? Then, is it true, as most Indian writers say, that leprosy is rare in Burmah at the present day, while authors within the present century have described it as very common, and as being treated by the Government by strict isolation? It would be most important to know whether the disease had diminished, and whether its diminution can be regarded as the consequence of this isolation.

While we agree with Dr. Carter on many points, and consider that he is quite right in following what is now the usual practice in dropping the name of elephantiasis for that of leprosy, we cannot help expressing our regret that his style is not more natural.

When we come on the heading of a chapter, "Prevalence in Time and Space," we begin to wonder whether we are reading a medical or a metaphysical description.

We wish Dr. Carter every success in the investigation of the interesting subject to which he has so long devoted his abilities.

Longmore's Instructions for Army Surgeons.[1]—The substitution of the rifle for the smooth-bore musket in the army naturally led to endeavours to utilise to the utmost the increased powers of range and precision of fire which the new arms possessed, and the efforts made to accomplish this soon brought to light many defects of vision in the men who were required to use it which had not forced themselves on the notice of the military or medical authorities previously. About fifteen years ago this question had become so pressing that Sir James Gibson, then Director-General of the Army Medical Department, requested Mr. Longmore, professor of military surgery in the Army Medical School, to prepare concise and simple instructions for the practical examination of the vision of recruits and soldiers, for the guidance of medical officers in this new but essential part of their duty. The result was the first edition of the 'Manual,' which appeared in 1863. That has been out of print now for about seven years, and the author, notwithstanding many applications to bring out a new edition, has hitherto, in presence of the works of Donders and others, hesitated to undertake it. The importance of providing the candidates for the army and navy medical services, while prosecuting their studies at Netley, with the necessary information as to the defects of vision they may subsequently require to deal with, in a concise form, has at last led him to revise and extend the former edition, without, however, his wishing the one now under consideration to be considered as more than an aid to the memory, and not as in any way obviating the necessity for the study of the larger works of the leading ophthalmologists by those who wish to become thoroughly proficient in the subject.

The manual is divided into two parts, the first of which contains five chapters. Of these the first, after explaining the terms in ordinary use, points out the various descriptions of lenses with their properties. Here there is an absence of illustrative diagrams. We quite understand the author's anxiety to compress his work as much as possible, as well to retain it within small compass so as to keep it moderate in price; and, so far as those who have had the advantage of attending his lectures are concerned, the absence of diagrams may be of little consequence, as no doubt such aids are freely used to demonstrate everything capable of being more forcibly conveyed through the eye than by mere description; but at a distance, and for those who are imperfectly acquainted with the effects of convex and concave lenses, a few simple diagrams showing the influence of a refracting medium in the course of a ray of light, and how the dif-

[1] *Manual of Instruction for the Guidance of Army Surgeons in Testing the Range and Quality of Vision of Recruits, and in Distinguishing the Cause of Defective Vision in Soldiers.* By Surgeon-General T. LONGMORE, C.B. 2nd ed. London, 1875 (pp. 134).

ferent manner in which the medium is presented to the incident ray in these, respectively, produces such a different result, would render this question, which is so intimately connected with all that follows, far clearer to the reader, and enable him to grasp the principle concerned at the outset.

In the second chapter the character of natural vision is defined, and the deviations from it, owing to the excess or deficiency of the refractive powers of the eye, pointed out, with their causes and the mode of distinguishing them and of determining their degree. The accommodating function of the healthy eye to distinct vision of objects at different distances is also treated of, and the influence of age and other causes in limiting its range clearly indicated.

The third chapter contains remarks on the acuteness of vision, with notices of the means ordinarily employed for determining its degree, and the nature and extent of the various deviations from it. In each instance, after a short definition of the nature of the particular form, its causes are pointed out, and the symptoms and diagnosis follow. The fourth chapter is occupied with a description of the test dots employed for determining the visual powers of recruits, and the principles on which they are based; and in the fifth the mode of conducting the examination is indicated, and the details in the previous chapters applied.

The second part is devoted to the ophthalmoscope and its uses, and contains three chapters. Of these the first embraces a description of the instrument, and the various details to be observed in employing it; the second gives a short sketch of the natural appearance of the various parts of the interior of the eye as seen with the assistance of the ophthalmoscope; and the third treats of the lesions of those parts discernible by the ophthalmoscope, and the means of distinguishing them. The manual ends with a copious index, which enables the reader to refer to any subject in the text with ease and readiness.

Though written with an especial reference to the requirements of medical officers of the army and navy, Mr. Longmore's manual, carefully prepared as it has been, and presenting in a compendious form much information that is diffused through many bulky treatises, is calculated to be of much service to medical men in civil life who may have to deal with affections of vision in the course of ordinary practice, but whose avocations do not leave them sufficient leisure to study the subject at length in the systematic works of original investigators.

Archives of Electrology.[1]—This journal, edited by the inde-

[1] *Archives of Electrology and Neurology.* Edited by G. Beard, M.D. Vol. i, No. 2, 1874. New York.

fatigable Dr. Beard, appears twice a year, in May and November, and in the present active state of neurology materials ought to accumulate rapidly enough for two parts per annum. At the same time we think it is unwise in an editor to tie his hands, and to oblige himself to bring out the parts of a serial at a given date whether he has good matter or not.

It is better to issue the parts at irregular intervals as material accumulates. In these days the multiplication of serials is a great hindrance to the inquirer, and to add to the bulk and dispersion of good matter by padding is too vexatious. Much of the present volume is padding of an indefensible kind, which must so far keep down the character of the publication. The editor himself, who gives due time and care to an article on chronic alcoholism which is well worth printing and reading, is obliged to eke out his space by some very flimsy "Letters to Inquiring Practitioners." Hungry practitioners surely may master rather stronger meat than this. The first place in the number is given to an article by Dr. Tripier, to which we take objections of a wholly different kind. He largely undertakes the treatment of uterine disturbances by Faradism, a process which requires the application of an electrode to the inner generative organs of women daily, or at least on alternate days, for weeks together. He also proposes to insert his electrodes into the bladder or rectum of maiden ladies, in whom daily vaginal explorations might have inconveniences. Now, from beginning to end of this article it seems never to enter this person's mind that such proceedings are only to be justified on the grounds of absolute necessity—on the grounds, that is, that this treatment has a value as far transcendent as its meddlesomeness is excessive. We need scarcely say that he shows nothing of the kind, nor is alive in any degree to the consequent gross impropriety of his proceedings. It is time that the profession should say plainly that this fashionable trifling with womanly delicacy is going too far—that too many physicians of late have been tempted to forget what is due to their own honour and to the proper reticence of their patients. Suave compliance on the one hand, and half-concealed pruriency on the other, may in time become too plain in both doctor and patient if the thing is not sharply checked by public opinion. Of late murmurings of no uncertain kind have been making themselves heard in many places. Of the rest of the number we can speak with more satisfaction. Dr. Munson takes up a subject on which we are far too ignorant, namely, the effects of lightning upon the nervous system. Dr. Frank deals with the electrolytic treatment of hydrocele. Dr. Seeley writes on galvanism in ocular and aural affections; and Dr. Bulkley takes up the relations of the nervous system to diseases of the skin. This opens up a very promising field, a far more promising one than the view which attributed many

such lesions necessarily to the blood *because* they are symmetrical! The number ends with editorial gleanings and notices of books. We heartily welcome the 'Archives,' and wish them something more than a *succès d'estime.*

Leroy-Dupre on Hydropathy.[1]—In this pamphlet Dr. Leroy-Dupré has laid before his readers a short account of the principles and practice of hydrotherapeutics. He commences with a description of the various methods in more common use, and of the apparatus necessary for the purpose; he next discusses the action of cold water upon the organism, and this is followed by an enumeration of the conditions the observance of which will result in a good *reaction.* Indeed, in the production of this reaction, due to the dilatation of small arteries which have contracted upon the application of cold, appears to lie nearly the whole secret of hydropathy; and with its production at the right time and in the right place the author is engaged in the succeeding sections on the therapeutical indications for its use and on the diseases requiring it.

Chapters on the contra-indications, and on its employment in private, conclude the treatise.

On the whole it presents a fairly comprehensive account of the methods and objects of hydropathy, which may be of service to those who wish to have more information on its practice than can be found in our works on medicine and therapeutics.

Milton on Vapour Baths.[2]—The main object of this small treatise is to introduce to notice a portable apparatus to administer vapour baths and modified Turkish or hot-air baths, invented by the author. Judging from the diagrams, it appears a sufficient, simple, and effective apparatus for the purpose intended; but we cannot quite free our mind from the impression that the individual employing it runs some risk of being enveloped not only in vapour but in flame, particularly if the Russian lamp happened to be overcharged or by some mischance upset. But such things are not intended for clumsy people.

The first section of the book is occupied by a short narration of cases for which the vapour and Turkish baths have proved especially useful. In lepra and eczema vapour baths have proved in the author's hands most beneficial. In prurigo Turkish baths have succeeded better, but neither form of bath is entirely to be relied upon in ichthyosis and sclerema. In gout and subacute rheumatism complicating skin diseases, both forms of bath are of great value. Neuralgic complications may be also expected to get relief.

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[1] *Des Indications et des Contre-indications de l'Hydrothérapie.* Per M. le Dr. Leroy-Dupré.

[2] *On the Modified Turkish and Vapour Bath, and its Value in certain Diseases of the Skin.* By J. L. Milton. London, 1875.

The medical practitioner in search of bath-apparatus for domestic use will do well to read the account of Mr. Milton's invention. The few cases of skin diseases recorded in this small volume treated by baths will also encourage him to essay their employment.

Nomenclature of Disease.[1]—This is an American issue of the 'Provisional Nomenclature' of the Royal College of Physicians of this country. It is most gratifying as well as complimentary to the English compilers of the nomenclature to find it thus adopted officially by an important branch of the public medical service of the United States, viz. the Marine Hospital Service. Its adoption is to be credited to Mr. Woodworth, the energetic supervising surgeon of the department, to whom we have been indebted for copies, from time to time, of the interesting medical reports of the Marine Hospital Service.

We learn from a prefatory note to this volume that, previously to this its first official adoption, the classification of diseases in the statistics of mortality for the Ninth Census of the United States was made, in all essential particulars, in accordance with it; that it is practically adopted by the Medical Department of the United States Army, and has been generally sanctioned by the profession in the States. "From the foregoing [remarks Mr. Woodworth] it will be seen that its promise to become the common nomenclature and classification for at least all English-speaking peoples is in a fair way to be realised"—a consummation much to be desired in the interests of medicine, both as an art and a science.

Journal of the Chemical Society.[2]—Some quarter of a century ago the physiological and pathological branches of chemistry numbered among their most ardent votaries some prominent British chemists. For various reasons other branches of the science of chemistry have since presented superior attractions, and medical chemistry has been in disfavour. It is, then, scarcely to the pages of the journal of our Chemical Society of London that we should turn expecting to find a full record of chemistry in its relations to medicine. But though our Continental *confrères* have been of late the chief workers in the fields of physiological and pathological chemistry, no completer record of the new discoveries in those departments, and indeed in every other branch of the science, will be found than in the pages of the 'Journal of the Chemical Society of London.' Up to the end of the year 1870 the journal contained only such papers as had been read

[1] *Nomenclature of Disease prepared for the Use of the Medical Officers of the United States Marine-Hospital Service.* By the Supervising Surgeon, John M. Woodworth, M.D. Washington, 1874.

[2] *Journal of the Chemical Society,* 1871–4. London.

before the society, either *in extenso* or in abstract; but with the commencement of the year 1871 the journal entered upon a new career of usefulness. The happy idea was conceived of supplementing the proceedings of the society by full and carefully prepared abstracts of all the current foreign literature of chemistry. How well this has been done can be seen by a reference to the journal itself, which appears every month under the able editorship of Mr. Henry Watts, aided by a large staff of abstract-makers, whose names are a guarantee that the work will be thorough, and who are supported by an energetic publishing committee.

It is not too much to say that in the volumes of the journal the physiologist, the chemist, and the physician will find a faithful record of all that is going on in this country and abroad that is of interest in the chemical world. Unimportant memoirs are briefly summarised, whilst those which are of greater interest are presented with some amount of detail. In all cases the length of the paper is noted, and accurate references given to the original sources. French, German, Italian, Russian, American, and English chemical literature is thus laid under contribution for the benefit of English chemists.

Wisely, as we think, the council of the society has resolved to bring their now very useful journal within the reach of all. Although the monthly numbers are not sold separately, the journal is supplied to any one on payment of a small annual subscription. We advise all our readers who are interested in scientific medicine to become subscribers; and certainly no scientific or medical library can be considered complete without the admirable 'Journal of the Chemical Society of London.'

Ziemssen's Medical Cyclopædia.[1]—It was only in the number of this Review for April last that we noticed at some length the German edition of this very comprehensive 'Cyclopædia of Medicine,' as far as it was then published. We have now before us three bulky volumes containing the articles then reviewed rendered into English by several American physicians, some of whom are well known in this country both by their position in the profession and by their contributions to medicine. All that remains us as necessary on the present occasion is to signify our opinion of the manner in which the translators have performed their work, and on this matter we have to pronounce a very favorable opinion. There is certainly an inequality of performance, the English idiom being much better conveyed by some of the translators than by others. Indeed, it strikes us that some few of the number have been selected for the work on account of their exact knowledge of German, but who, haling from "the Fatherland," have not hitherto acquired the

[1] *Cyclopædia of Medicine.* Edited by Dr. H. von Ziemssen. Vols. i, ii, and iii. London, 1875.

English idiom. The prospectus issued in this country gives a list of "gentlemen who have agreed to take part in the international work of translation," and who are more or less known to us as fellow-countrymen in practice in Great Britain and in Ireland. But so far as the volumes as yet issued are concerned, these translators on this side the Atlantic have not put in an appearance. All those whose names are appended to the articles now before us are physicians residing in the United States.

In fact, the publication is an American enterprise, of which the London house of Messrs. Sampson Low and Co. make themselves the medium to place before the profession in England. The production of a cyclopædia on the scale on which this present one is projected indicates a strong conviction on the part of the American editor, Dr. A. H. Buck, of New York, and his collaborateurs and publishers, of the value of the work and of the desire of the profession to possess themselves of so complete a survey of German pathology and practice. In this country it has a worthy competitor in 'Reynolds's System of Medicine,' and will find it no easy task to make its way into the libraries of English practitioners, and especially of such among them as have a strong prepossession in favour of English "practice."

The publishers refer to it as an "international" work, but we do not quite recognise this to be its character, seeing that it is wholly a production of Germany. But, being now translated into English, it so far becomes international that it conveys the teaching of Germany to the many peoples to whom English is the native tongue, and nothing can more conduce to the advancement of medical science than the diffusion among different nations of the principles and practice prevailing among them individually. An intercourse of opinions is as advantageous in medicine as in social science, by dispelling narrowness of views and overturning self-opinion and self-conceit.

Those who possess themselves of these volumes may be congratulated on obtaining most painstaking, scientific, and complete articles on the subjects treated of in them. The type is large and clear, highly commendable to eyes past their prime, and the general "get up" of the volumes is most excellent.

Agricultural Report of Victoria.[1]—We have been favoured with a copy of the report of the Secretary for Agriculture in Victoria, which may be commended to all intending emigrants to that colony, to those interested in the history and progress of our great English colonies, to our naturalists, and even to our agriculturists, who may get a wrinkle on farming matters from their fellow-workers in the

[1] *Second Annual Report of the Secretary for Agriculture, Department of Lands and Agriculture, Victoria.* Melbourne, 1874.

antipodes. The naturalist will find a very good account, with excellent figures, of some Victorian and other Australian grasses, by Mr. Bacchus; and also a description of some Australian wood-boring beetles, also illustrated. The meteorologist has presented to him a report on the meteorology of Victoria, with numerous tables to gladden his heart, setting forth from day to day the state of the barometer, of temperature, of dew-points and humidity, the prevailing direction of the wind, and the amount of rainfall, accompanied by various notes and comments from the able observations of the Government Astronomer, Mr. Ellery, F.R.S.

Intending emigrants who patronise beer and wine will be gratified to learn that both hops and the vine are extensively cultivated in Victoria; the latter important plant occupying a surface of 200 acres, and promising to become a source of much wealth to its cultivators.

Another important industry has been introduced into the colony since 1873, viz. "Sericulture," or the growth of silk. As a matter of course it is yet in its very infancy, but the silk exhibited at the Vienna Exhibition was pronounced equal to the best Italian. The cultivation of flax has also been attempted, but climatic conditions are unfavorable to it. Nevertheless there is a earnest endeavour to overcome difficulties and to win success. The volume also contains several papers of interest to farmers and horticulturists; and it will hereafter, apart from its present intrinsic value, be of much importance in tracing the industrial history and progress of this most enterprising and successful colony of Victoria.

Salt on Electrical Apparatus.[1]—This small book is no superfluous addition to our stock of treatises on medical electricity. It supposes the reader to know the principles of the application of electricity in the treatment of disease, but not to be equally familiar with the results of the inventive faculties of mechanicians ever seeking after perfection in the construction of instruments. And as no one can be presumed to be better acquainted with the mode of dealing with their instruments than the makers of them themselves, the profession is indebted to the well-known instrument makers of Birmingham, the Messrs. Salt, for the publication of the brief volume under notice.

The meaning of the printed text is greatly elucidated by numerous excellent engravings of apparatus and of portions of apparatus. The book is well printed, and at the moderate price put upon it may be secured by every practitioner of medicine who does not consider himself an expert in the use and management of electric and galvanic batteries.

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[1] *A Practical Description of every Form of Medico-Electric Apparatus in Modern Use, with Plain Directions for Mounting, Charging, and Working.* Illustrated by carefully drawn Engravings. By SALT and SON, of Birmingham. London, 1875.

Original Communications.

I.—On Lung-Disease from Inhalation of Dust.—By John T. Arlidge, M.D., A.B. Lond., F.R.C.P. Lond., Physician to the North Staffordshire Infirmary, Stoke-upon-Trent, &c.

Additional evidence is constantly accruing to prove that dust may be inhaled, that it may penetrate to the lung-cells, and that its presence within the pulmonary tissue is a cause of serious organic disease. The older medical writers have recorded not a few cases illustrative of these facts, and it remained for the physicians of the generation now passing away, and of the present one, to cast serious doubts upon them, and even to deny the possibility of minute particles of foreign matters making their way within the lungs by inhalation. This denial had a physiological basis, viz. :—that the air-passages had ciliated epithelium, and that the action of the cilia thrust forward and outward any particles entering within those passages, and thereby prevented their further penetration towards the pulmonary tissue. It likewise rested on the assumptions, that ciliary action must always be in the same direction; that it, so to speak, could not be wearied by long-continued and persistent inhalation; that it could not be enfeebled by habits of life and other causes destructive of vital power; that its resistance could not be overcome by violent inspiratory acts involved by exertion or coughing; and that inhaled particles could enter the lung-tissue only by a progressive passage through the repeatedly branching bronchi.

The physiological doctrine of the impeding action of the cilia to the entrance of foreign particles into the lung-cells is undeniable; but experiment and observation prove it not always and under all circumstances effectual. Direct experiments upon animals, made by Lewin[1] and others, demonstrate this inefficiency; and the records to be met with, in works on forensic medicine, of deaths consequent on suffocation in an atmosphere laden with particles of carbon,

[1] 'Die Inhalations Therapie,' Berlin, 1865, p. 87 *et seq.*

equally prove that the resistance of the cilia can be overcome. The several assumptions mentioned are equally disposed of by more minute observation and reflection. Physiologists have shown that the direction of ciliary action admits of variation, that its activity and power are in direct relation to tissue-integrity and to vital energy, and that the simple onward course through the tubes to the alveoli is not the only way that foreign particles can penetrate the tissue, but that they may enter by medium of the mucus-corpuscles which invest them, and may likewise find their way into the lymphatics; lastly, and without contention, that foreign matters in very tangible quantities are found in the bodies of individuals exposed to the inhalation of dust, and that the quantity discovered exists in a far higher proportion among those so exposed especially than among other persons. The disbelief in the interpenetration of lung-tissue by inhaled dust prevailed principally among those less familiar with occupations highly productive of dust. For instance, it obtained chiefly in the German and French schools; in the former under the influence, of late years, of that most distinguished pathologist, Professor Virchow. The French objectors were also ably led by Andral and Trousseau. In England, on the contrary, the special region of coal-mines and of manufacture, the inhalation of dust, and the production of lung-disease thereby, has been always generally held as an established fact.

The opponents of the doctrine, however, did good service by showing that black discoloration of the pulmonary tissue, with and without induration, enlargement and blackening of the bronchial glands, and nodules of apparent gritty or sandy matter, may have their origin in morbid changes quite independently of inhaled matter. Heusinger and Becker attribute the black staining or granular masses they encountered to defective elimination in the tissues of carbon and carbonic acid, and to a sort of precipitation of carbon within them; whilst Hasse thought the black infiltration to be, on the one hand, associated with a process of healing in lung-lesion, as for example tubercle, or, on the other, to be connected with changes incident on age, as illustrated in senile atrophy of the lungs. Virchow, again, sought an explanation of it in the hypothesis that its origin was in the altered colouring matters of the blood; and in evidence referred to cases of hyperæmia of the lungs consequent on heart-disease, accompanied by diffused extravasions varying in hue from red to black. In such lesions the altered coloured matter is, for a time at least, included within cells, but these disappear, and the pigment may be set free, and be then encountered in the interstices of connective tissue in the form of black granules, and more rarely of crystals. He further assumed it as probable that the production of certain coloured matters in particular tissues might be influenced by some special nutritive qualities of those tissues, or by individual

peculiarities, just as seems to happen in the coloration of the hair and of the dermis.

On the part of French physicians, Trousseau and Leblanc entertained similar views with Virchow.

The interpretation supported by the foregoing eminent pathologists is doubtless right enough with regard to many examples of blackened lung-tissue. Wherein they erred was in making that interpretation exclusive. Their experience apparently never brought them into contact with cases,—common enough in some manufacturing and coal-getting districts, to which their explanation would not apply, and which, both by histological appearances and by chemical analysis, would have demonstrated to them the presence of inhaled foreign particles, and not only altered and disintegrated blood effused within the lung-parenchyma as a consequence of disease.

A case decisive of the fact that foreign solid matters may enter within the lung-tissue by breathing occurred to Traube in 1860, and is appealed to by German authors as the first placed on record so demonstrative. However, it is quite certain that many English physicians had convinced themselves of the occurrence, long previously, by post-mortem examinations; nevertheless Traube's case is highly valuable by reason of the minuteness with which it was examined, and the care and fulness with which it is reported. A serous black fluid, staining the fingers, could be squeezed from the affected lungs, which, on microscopical examination, presented granules precisely resembling those of charcoal-dust, in which the patient had for many years worked. Moreover, small pieces of dust were found at different depths in the epithelium, some of which stuck in like arrows, whilst others had entirely penetrated, and others again had transfixed the cells and were advancing into adjoining cells. Nay, Rindfleisch,[1] who assisted in the examination of Traube's case, assures us that he found in the juice squeezed from the parenchyma of the lung "one of the dotted cells of coniferous wood entirely carbonized, but in which I was enabled to count seven pores close together. This particle of charcoal-dust equalled half the diameter of an alveolus."

That dust may enter, in the act of breathing, into the bronchi, the alveoli, and the parenchyma of the lungs, must therefore be accepted as an established fact. Whilst lodged within the bronchial tubes, the atoms of dust are, however, liable to be propelled outwards by the cilia, and to be ultimately expectorated. But those that escape this salutary action penetrate into the alveolar parenchyma, where they are no longer exposed to disturbance from the cilia, but now follow, as Rindfleisch describes, "the general current of the extra-vascular nutrient fluid, together with

[1] 'Pathological Histology,' Syd. Soc. edition, vol. ii, p. 52.

which they tend ultimately to reach the lymphatic vessels. On their way they must occasionally meet with corpuscular elements which have the power of permanently adopting small solid particles into their protoplasm. Foremost among such elements are the stellate corpuscles of the connective tissue; next, the migratory amœboid cells which are found in the connective tissue of the lungs as well as elsewhere, and which carry the black pigment with them wherever they go. The residual portion, that which escapes being arrested by cells on its way to lymphatics, is carried to the root of the lung and enters the lymphatic glands of the mediastinum, and here the granules meet with an insuperable obstacle to their further progress, for the countless lymph-corpuscles with which the glands are stored are ready to take up as many of the charcoal-particles as can, by any possibility, be accommodated in their protoplasm" (op. cit., p. 51).

From the preceding observations it appears that the lungs may be discoloured, and seriously embarrassed and injured in structure and functions, by inhaled particles; and further, that they may be somewhat similarly discoloured by altered blood-matters, the resultants of disease within the pulmonary tissue itself, and which, in their turn, may be associated with, or act as the effectual causes of, other disease. The term melanotic lung has been loosely applied to both forms of discoloration; it would be well if disused altogether, for it seems to imply a certain substantive morbid condition of lung characterised by a black colour, whereas it is evident that colour is an accidental feature dependent on differing causes.

There is not much danger of the two primary varieties of discoloured lung being confounded either during life or after death; their history so widely differs, as does also, I may add, generally their pathological appearances. Of this more hereafter.

The motes in the sunbeam have made man in all ages cognisant of the diffusion of minute particles at all times in the air we breathe; but the enormous extent of their diffusion has been much more vividly brought home to our minds by the admirable experiments of Prof. Tyndall; and what is more, these experiments exhibited the constituent parts of the atmospheric dust, showing that some portion of it was composed of organic, the rest of inorganic matters. Any one who realises the fact of the enormous diffusion of dust in the air will less marvel at the fact that chemical analysis has shown the presence of mineral matter in the lungs of children within three or four years after birth, and, *cæteris paribus*, the progressive accumulation of such material in the lungs with advancing age. It is in the form of silica that the mineral dust has been sought after by analysis. In illustration I may quote the results obtained, very recently, by Riegel in conjunction with his colleague Dr. Hauser (op. cit., p. 222). The lungs analysed were those of individuals not

subjected to conditions of life and labour involving exposure to more dust than common. The lungs of a child, four weeks old, furnished the first example. In them no silica was found. In those of a boy, æt. 4, silica constituted 2·44 per cent. of the ash left after incineration. In those of a day-labourer, æt. 47, it amounted to 13·39 per cent., and in those, again, of a woman cook, sixty-nine years old, it reached 16·69 per cent.

Accepting these figures as accurate, they leave no doubt of progressive accumulation in proportion to age among individuals similarly situated. Common observation, moreover, speaks to the same fact, for a blackish discoloration of the lungs is a feature which arrests attention, particularly in those who are more advanced in life.

As might well be anticipated, in various occupations in which much dust is produced and thrown off, its inhalation becomes so considerable that sooner or later the foreign particles permeating the bronchial tubes and lung-tissue become a source of suffering and disease. In some departments of labour pulmonary disease has been so distinctly due to the inhalation of a particular dust generated that it has received special appellations from the occupation pursued, such, for example, as "miners' phthisis," Sheffield "grinders' rot," "potters' consumption and asthma." In some other forms of labour the dust by inhalation has long been popularly known as a cause of disease; for instance, among freestone hewers and masons, among the operatives in cotton and woollen mills, and among millers. In a less degree many other workmen engaged in other pursuits suffer from the same morbific agent. Lewin refers to charcoal-grinders and carriers; to chimney-sweeps, moulders, and iron-polishers; to gas-stokers, to smiths, and glass-polishers; and the list might be increased by quotation of smaller trades, foremost among which would stand workers in mother-of-pearl, the fatality amongst them from pulmonary disease being probably unequalled in any other class of artisans.

Now, a great difference obtains in the extent and severity of the disease produced from the character of the dust inhaled; and it may be safely predicated that mineral dust, as that of freestone, of potters' clay, of coal and mother-of-pearl, is much more destructive of lung-function and tissue than is the dust of organic origin, as that of wool, or cotton, or flour. The amount of suffering will also bear a direct relation to the character of the industrial processes pursued, in some of which the generation of dust is more abundant, and its inhalation favoured by the circumstances of labour. For example, the heated air of workshops, or difficulties in the way of ventilation imposed by special conditions of labour, the association of the dust with particular vapours or gases, may be well imagined to add to the intensity of the evils of dust. To go into particulars upon these points would lead me on to an essay on noxious trades. But this much it is necessary for me to say, that differences in the nature of the

dust inhaled, and differences in the circumstances attending its inhalation, serve to modify somewhat the symptomatic features and history of the consequent disease during life, and likewise the pathological appearances after death. But, granting such differences, I can express no admiration for the prolific word-making of some physicians, especially among the Germans, to describe those different trade-propuced chest-affections. Thus pneumonokoniosis stands for diseases due to the inhalation of dust in general; anthracosis for those due to coal-dust; chalicosis for such as arise from siliceous stone-dust; siderosis for ferruginous dust-disease. These, and other like barbarous words pointing to other varieties of dust, I should be very sorry to see perpetuated; and the more so as I hold that, whatever dust be inhaled, the pathological process set up by it partakes of the same essential character, though differing in intensity, and, in the case of inhaled organic particles, somewhat modified as to results produced. As my experience of lung-disease caused by the inhalation of dust has almost entirely fallen among miners and potters, I shall confine myself to the description of the lesion met with among such workmen. And first, let me state that lung-disease is more prevalent and more fatal among potters than among miners. I consider the dust from the clay and flint used by the former to be more detrimental to pulmonary tissue than coal-dust. It is essentially a siliceous dust, and is rendered extremely fine by the processes the clay passes through in its preparation. Moreover, it is worth noting, that in several processes in the manufacture of pottery, particularly in the making of china, finely ground flint is largely used, either in a pure state or mixed with china-clays.

As a siliceous dust, made up of more or less angular atoms, it is naturally extremely irritating to the tissues, and may be supposed to have more penetrating power than other kinds of dust, by its hard, vitreous character. The German writers note a difference in the power of penetration between charcoal-dust and the dust of mineral coal, remarking that the former makes its way more readily into the tissues than the latter, by reason of its possessing, more or less, the form of spiculæ, or elongated sharpened splinters.

But, at the same time, though the clay-dust may pierce the epithelial surface more readily than the coal, it does not exhibit the same diffusibility; perhaps by reason of its more irritating character and the consequent inflammatory action provoked. For, on the whole, the coal-dust penetrates the lungs and discolours them more generally than does the clay-dust. At the same time both forms of dust exhibit a preference for the upper and posterior portions of the lungs; in other words, the dust accumulates most in those parts first reached in the inspiratory act. The lungs assume a black or grey-black colour wherever the deposit takes place; and, as is natural, the depth of colour is greatest where coal-dust is the

substance inspired. Generally speaking, the depth of colour is greatest where most condensation of tissue is found; where the particles of dust are more diffused the colour is rather greyish black. The condensation of lung is as remarkable as the black colour. Hardened masses occur, at times measuring two inches and upwards in length and width, and in depth or thickness nearly as much. Sometimes they have a somewhat rounded configuration, especially when of smaller dimensions. But under no circumstances are they at all separable from the surrounding tissue as distinct heterologous tumours. They have no defining line, but the condensation of tissue progressively lessens from the centre of greatest density, the lung-substance around exhibiting signs of the spreading lesion. A section of the condensed mass presents great resistance to the knife, cutting with a leathery or cartilaginous consistence, and now and then with a gritty sensation. When first cut through the colour is pretty uniformly black, but, after washing, the surfaces present lighter and darker specks, and, to find a similitude, have a nutmeg-like aspect. No blood exudes on its section, but the pulmonary tissue around is hyperæmic. It is common also to find an emphysematous state of neighbouring lobules. This condition is most pronounced along the anterior edges of the lungs.

The indurated portions for the most part reach the surface, and are there covered by a thickened pleura, and pleuritic adhesions are usual.

In some cases softening in the centre of the condensed tissue has taken place and dusky pus or a purulent detritus is formed within it. In many instances tubercular deposits are found associated in the same lungs with the hardened masses. I have not observed caseous matter in the condensed tissue, but there may be caseous tubercle in the same lungs, breaking down and forming abscesses as in ordinary consumption.

Together with the hardened state of the pulmonary parenchyma, there is always more or less chronic bronchitis. The bronchial secretion is ordinarily viscid, of a yellowish or yellowish-white colour; but its consistence and viscidity vary according to the presence or absence of activity in the bronchial inflammation and in the diseased parenchyma. In the earlier phases of the lesion, and when the men are still pursuing their occupation, more or less black, sooty-like particles discolour the expectoration. This blackening of the sputum gave rise to the term "black-spit" used formerly by miners to designate the form of bronchitis that especially attacked them. It is, indeed, a phenomenon observed in a more intense degree among miners than among potters, though present in each class of workmen. When a miner or potter is attacked by severe catarrh or bronchitis the black spit is particularly observed

at the beginning of his illness; as his illness goes on the discoloration lessens, the collection of dust in his bronchial tubes having been mostly got rid of.

Enlargement, induration, and blackening of the bronchial glands about the roots of the lungs is a lesion universally encountered along with condensation of lung from dust-inhalation. When speaking previously of the course pursued by the dust this glandular change was remarked on. There is abnormal growth of the connective tissue both of the glands and of their capsules, and in the end destruction of the true glandular substance.

The microscopic appearances of the indurated connective tissue and limited pneumonic activity, with excessive growth of black lung, bespeak a hyperplasia. The inhaled dust acts as a local irritant in the parenchyma of the lung, surrounding the alveoli, and likewise upon the lining membrane and walls of the bronchi. Lymph-exudation takes place; encroaches and presses upon the air-cells; develops partially into fibrous tissue, whilst other portions do not advance beyond a granular stage. The lymph-matter contracts in the course of its growth; the alveoli are destroyed, and their vascular supply is cut off. The contraction of the new tissue is evidenced to the naked eye by the puckerings of the diseased mass, although this is sometimes masked by the free development of a loose connective tissue around and by pleuritic thickening.

The action of acetic acid clears somewhat the mass and brings to light in it—which may previously have looked like a formless granular mass—some recognisable remnants of lung-tissue in the shape of elastic fibre and rude broken outlines here and there of former air-cells. The black matter is seen in the shape of more or less rounded specks, scattered loosely or gathered in broken lines, or collected into irregular heaps amid the fibroid tissue. When seen in lines these black particles can generally be made out to be present in the connective tissue around the former alveolar cavities, or else spreading along in the direction of lymphatics.

Judging from appearances in the diseased tissue after the action of acids, I infer that not only is lymph-matter thrown out in the septa between the alveoli, but also that it occupies the air-cells, and conspires with that exuded around them in their obliteration.

The whiter dots seen on section of the hardened lung are shown by the microscope to be small bronchial tubes; and it is noticed that the walls of these tubes are greatly thickened, and that their lumen is obliterated. When nitric acid is applied to a fragment of the diseased tissue it appears at first to harden and corrugate it, but its prolonged action induces softening and destruction of all structure. So soon as the acid acts upon the tissue a lively effervescence ensues indicative of the presence of a carbonate, in all probability carbonate of lime. After the acid has done its

work many particles remain, among which are seen some having the appearance of minute splinters of silica.

The chemical analysis of diseased lungs has not been prosecuted so extensively as it should have been. Although by no means numerous, it would be useless to give the results of the analyses published by various experimenters. Kussmaul made special examination respecting the inhalation of siliceous sand by quarrymen, and the general fact arrived at by him was, that in that class of workpeople the silica in the lungs exceeded by three times the amount in others of like ages not especially exposed to dust-inhalation. Another analysis by Meinel, quoted by Riegel, of diseased stonemasons' lungs exhibited a minimum of 23·3 per cent. and a maximum of 45·64 per cent. of siliceous and sandy matter in the ash of the incinerated lung. So Riegel's own analysis of the lungs of a stonecutter, whose case he relates, showed that the ash of the dried substance equalled 3·94 per cent., and that the proportion of silica in the ash was 41·38 per cent. In another similarly employed the ash amounted to 5·22 per cent. and the silica of the ash to 37·47. In two other individuals, also stonecutters, these same figures were respectively 4·58 per cent. of the ash and 38·48 of silica in the ash, and 5·57 per cent. of ash and 58·3 per cent. silica in the ash.

I may now subjoin the analysis of a condensed piece of potter's lung, most kindly made for me by Mr. A. H. Church, the Professor of Chemistry in the Royal Agricultural College, Cirencester. He found the specific gravity of the mass to be 1·06. It contained 76·37 per cent. of water, 20·91 of organic, carbonaceous, or volatile matters, and 2·72 ash of mineral matters. In 100 parts of the mineral ash silica amounted to 47·78, alumina to 18·63, peroxide of iron to 5·55, and alkalies and undetermined matters to 28·04.

The proportion of silica in this example of lung exceeded, therefore, that found in any previous analysis quoted, except one where that substance is stated to have reached 58·3 per cent. of the ash. But this analysis of potter's lung stands alone in the determination of the alumina present, an ingredient in the clays used in pottery, but not to be looked for in the lungs of workers in freestone.

The carbonate of lime in potter's lung, as attested by effervescence with acids, may be assumed to constitute no inconsiderable portion of the 28·04 per cent. of alkalies and undetermined substances noted in Prof. Church's analysis. This carbonate of lime will give increased consistence to the diseased tissue, and, at the same time, add to its opacity under the microscope, a circumstance attested by the clearing of specimens after the action of acids. Its origin is doubtful, but it may be supposed to be derived from tissue-elements themselves, carbonate of lime being a frequent con-

stituent of morbid products, for instance, in brain-sand, in obsolescent tubercle, &c.

The clinical history of indurated lung produced by inhalation of dust may be sufficiently told in a few words. The disease is a progressive one, although progress may be materially retarded by withdrawal from the occupation involving the inspiration of dust. Nevertheless, when once the morbid process has been set up, it would seem to act, through the affected portion, as a permanent cause of irritation; so that the lungs are prone to take on inflammatory action from the influence of cold and depressing circumstances of no great intensity.

The onset is not rapid. Men of fair constitution will work for ten or more years before exhibiting definite symptoms of lung-lesion. Patients will state that for one, two, three, or many winters they have had cough with expectoration, but that they have recovered on the accession of warm weather; and that each succeeding winter the cough has grown more obstinate and has receded more slowly with advancing summer. Now and then one severe attack of bronchitis appears to establish permanent mischief, and ever afterwards the patient continues an invalid. In all the earlier stages the symptoms are those of ordinary chronic bronchitis. The sputum varies according to the present condition of the patient, becoming viscid when there is a recrudescence of inflammatory action. At times it is very abundant and frothy, at others coughed up in the form of rather heavy pellets, or of irregular masses shaping themselves to the bronchial tubes. A greyish and milky opacity is common, and in rarer cases the expectoration is fetid. A purulent admixture is less observed unless there be at the same time tubercular disease. Hæmoptysis is very uncommon. If met with it affords good reason for suspecting the presence of tubercle.

A prominent symptom is the shortness of breath. Speaking generally, this symptom is out of proportion to the bronchitis present. In an ordinary bronchitic patient we should not expect to find it of equal intensity. In advanced stages of the disease the dyspnœa is extensive. The chest, nevertheless, does not get more rounded as it does in emphysema, but suffers contraction. The two forms of "consumption," however, are frequently united; when this is so we have, as a matter of course, the usual symptoms of pulmonary phthisis with an unusual amount of dyspnœa. Wasting goes on to a very extreme degree, just as in tubercular consumption, but without the same hectic and sweating. The aspect of the patient bears the closest resemblance to that of a phthisical patient, and the malady well deserves the popular name of "potters' consumption" and "potters' asthma." In fine, the distinction in many cases between "potters' consumption" and tubercular consumption is difficult to make. The deliquescence or breaking down of morbid deposit in

the one malady and in the other is an identical process; so likewise in pneumonic phthisis and in potters' phthisis there is alike a lobular pneumonia, but experience indicates that a suppurative process belongs to the former lesion rather than to the latter.

Frequent sharp pains in the chest, varying from time to time in position, are remarkably frequent in potters' consumption. I have already noted that the indurated portions of lung, as a rule, reach the surface and that the pleura over them undergoes thickening. Those pains, therefore, may be assumed to be very often pleuritic in character.

On the whole the temperature in potters' consumption does not range so high as in tubercular phthisis; and, as before remarked, sweating is of small amount or absent.

In the matter of physical signs it is worthy of notice that dulness on percussion is not always present where condensation of lung exists. Examination after death has shown this to be the case, and that an emphysematous state of surrounding portions of tissue may be accepted in explanation of the fact.

A considerable difference is observable in the power of resistance of different individuals to the ill-effects of dust inhalation. This difference is attributable to various circumstances affecting them and their surroundings. Whilst a good constitutional vigour may stand in good stead one artisan, a feeble, delicate frame, particularly if there be hereditary lung-weakness—which is extremely common among potters—will render another an easier prey to the ill-effects of his occupation. But a wide difference arises, also, from the workman's surroundings and the division of labour which he specially follows, for some departments of the trade are more detrimental to health than others. To go into particulars on this head would carry me far beyond the limits of this paper. Other collateral conditions, entailing variations in proclivity to disease, are to be found in the hygienic conditions of the workshops. In all shops where the clay is moulded and pressed the temperature ranges high, but in the worst of their number not only is there an unduly elevated temperature, but very defective ventilation, draughts, and dampness of floors and walls. In fact, in many old manufactories there are workshops totally unfit for occupation in a sanitary point of view.

But, unhappily, in too many cases the baneful effects of their trade, and of the conditions of its exercise, are aggravated by their own folly and vice. There is a large amount of drunkenness, dissipation, and irregular living, sapping the health, and rendering their victims an easy prey to disease, particularly to colds and inflammations of the chest, which are the invariable antecedents of the subsequent induration with its attendant asthma and wasting. Much might be done to lessen the dust given off in the processes of manufacture by

sanitary constructions, and the inhalation of dust might be, to a very large extent, avoided by the use of simple respirators. The former object will, it is to be hoped, gradually be arrived at through the intelligence and philanthropy of employers; the latter is principally in the hands of the workpeople, who could readily adopt the use of respirators did not their stupid pride and weakness of resolve, their indifference or opposition to whatever is new in their modes of work, and their ridiculous sensitiveness to personal appearance and the possible ridicule of others, stand in the way, and so make them sacrifice health and life to inexorable trade usages and foolish sentimentality. The operation of the Factory Acts has done a great deal towards the structural improvement of manufactories; has rendered work more regular, particularly by the stoppage of night labour; has limited the hours of work, and prevented the employment of children too young or too weak. But to bring about all the sanitary improvements, whether of manufactories or of those employed within them, a considerable extension and many amendments of the sanitary provisions of those Acts are needed.

The preventive measures of diseases arising from the inhalation of dust would afford matter for lengthened discussion, but such a discussion would be here out of place. I will therefore conclude this communication by remarking that I have preferred setting forth my own observations and the results of experience to writing a complete disquisition on the subjects considered, by examining and discussing the statements and opinions of many physicians who have dealt more or less fully with the like topics, otherwise it would have been my duty to have referred to several English authors who have written on the diseases of artisans, and especially to the painstaking and extensive inquiries of Dr. Headlam Greenhow.

II. Memorandum on the Presence of Air in the Middle Ear as a Sign of Live Birth. By F. Ogston.

In the 'Monatsschrift für Ohrenheilkunde,' 1868, Dr. Robert Wreden, of St. Petersburg, in a series of articles on the condition of the ear at and soon after birth, directs the attention of medical jurists to the entrance of air into the middle ear, and the disappearance of the gelatinous substance which fills the tympanic cavity prior and up to the birth of the child.

He states that this substance disappears within twenty-four hours after birth; that a twelve hours' respiration is not sufficient to effect its complete disappearance; and concludes by suggesting that the occurrence of air in this situation might be of importance in a medico-legal point of view, as proving that respiration had taken place.

That these statements have been made without sufficient grounds, and that they require considerable modification, the following fifteen cases, collected partly by Dr. Alexander Ogston and partly by myself, seem to prove:

	Age of child.	State of the middle ears.	State of lungs.	Cause of death.	Other facts to fix term of life, &c.
1	14 weeks.	Filled with air.	Fully expanded.	Smothering.	Bronchitis.
2	9 weeks.	Filled with air.	do.	do.	do.
3	2 months.	Filled with fluid.	do.	do.	——
4	6 weeks.	R. air, L. muddy fluid.	do.	do.	Bronchitis.
5	1 month.	Filled with air.	do.	do.	——
6	4 weeks.	R. air, L. air and fluid.	do.	Broncho-pneumonia.	——
7	8 days.	Filled with air.	do.	Bronchitis.	Lungs bulky, emphysematous.
8	3 days.	Containing air.	do.	Smothering.	——
9	2 hours.	Fluid and yellow substance.	Partly expanded.	Apoplexia-neonatorum.	Breathed feebly about 2 hours.
10	New-born.	Containing fluid.	Expanded.	Smothering.	Meconium in large intestine.
11	New-born.	Filled with red fluid.	do.	Fracture of skull.	do.
12	New-born.	Containing fluid.	do.	Smothering.	Navel-string attached. Meconium in large intestine.
13	New-born.	Containing red fluid.	Partly expanded.	(?)	Meconium in large intestine.
14	New-born.	Containing air.	Expanded.	Smothering.	do.
15	New-born.	Containing fluid.	Unexpanded.	Stillborn.	——

I ought, perhaps, to have omitted the first six cases as not bearing directly on the subject, but I have thought it better to adduce them, as though most of the ears contained air, yet some of them contained fluid also, and in one (3) they were filled with fluid, although the child had lived two months, and there were no signs of catarrh in the respiratory tract to account for its presence. The fluid was probably catarrhal; but as it was not examined microscopically I cannot state positively that it was so.

The last nine cases, on the contrary, are those in which Dr. Wreden's proposed test should be expected to prove useful, but, as we see by the table, in only three (6, 7, and 14) was air found in the middle ears, although the lungs in all, with the exception of a stillborn immature infant, contained sufficient air to float in water, and from all air could be expressed when they were held under water.

The presence of catarrhal fluid in the middle ear might be misleading, but in these cases more or less air would be found along with it which would tend to prevent its being mistaken for the gelatinous matter found before birth.—F. Ogston, Jun.

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.,
Fellow of the Royal College of Physicians, London, Physician to the Bloomsbury Dispensary, London.

On the Action of Eserine (Physostygmin) on Chorea. By Dr. Bouchut, of the Hôpital des Enfans Malades, Paris.—After some general observations on the physiological researches previously made on the active principle of the Calabar bean, Dr. Bouchut states, as the result of his recent investigations, that the action of this alkaloid is very different in man from that produced in dogs. He has employed the sulphate of eserine or eserine itself four hundred and thirty-seven times in the human subject, but not, of course, in poisonous doses, although he has carried it to the extreme limits of tolerance, in the dose of three, four, five, and six milligrammes (a milligramme is the $\frac{1}{1000}$ of about fifteen grains), which quantity he thinks it dangerous to exceed. Hitherto eserine has been employed internally only in a few cases of tetanus, but Dr. Bouchut conceived, judging from its physiological action, that it might be useful in chorea; and of the whole number of his cases, 205 took the medicine in the form of pills, and the other 232 were treated by subcutaneous injection, the dose of eserine being the same in all cases, namely, from two to five milligrammes. All the patients were children affected with chorea or nervous convulsions, and their ages were from seven to twelve. Dr. Bouchut in this elaborate paper shows in a series of successive paragraphs the effect of subcutaneous injections of pure eserine in different doses; the effect produced by the subcutaneous injection of sulphate of eserine; the effect of the sulphate of eserine in pills, and that of the same substance taken in solution; the action of eserine on the composition of the urine; the comparative action of eserine in man and in the lower animals; the effects of the alkaloid on the contraction of the peripheric vessels; and, lastly, the therapeutical effects of eserine and its sulphate in chorea and other convulsive diseases.

The injection of pure eserine in the dose of five milligrammes in children appears to be attended with very striking and rather serious consequences; for when twelve children, from seven to twelve years old, affected with chorea, were subjected to this treatment, they suffered

severe epigastric pain, nausea, slimy and scanty but painful vomiting, and more or less well-marked paralysis of the diaphragm, which rendered the vomiting more difficult. It is remarkable that the pupils were not strongly contracted in any of the children, but in two of them they were slightly contracted, and in all the rest they were dilated, although at the same time they were contractile.

The above symptoms lasted three hours, and during this time the choreic movements were arrested, but they returned progressively in ten cases in proportion as the eserine lost its action. In smaller doses, employed in injections, the symptoms produced were similar but of a slighter character, and the chorea was relieved during the whole time of the action of the eserine, with one exception, among eight children. When the alkaloid was administered in the form of pills the symptoms produced were less marked than when the subcutaneous injection was employed; so that by the former method three to five milligrammes might be given with safety, while it was dangerous to exceed two to three milligrammes by the latter. As to the presence of albumen and sugar in the urine under the influence of sulphate of eserine, Dr. Bouchut has never been able to find any such change; and although he does not dispute the fact that these substances have been found in the lower animals treated by this drug, yet he suggests that the action is probably different in man. With regard to the time when the symptoms commence, Dr. Bouchut has found that after injections the effects are almost instantaneous, some of the little patients becoming pale and sick, spitting frothy or watery but scanty matter from the mouth, or making attempts to vomit which are prevented by the weakness of the diaphragm. The paralysis of the last-named muscle is the most disagreeable result which ensues after the injections of large doses of eserine, as, for instance, five milligrammes; and even asphyxia might ensue, although below three milligrammes this is not likely. An unexpected result was observed by Dr. Bouchut, namely, that the pupil remained tolerably contractile to the light and seemed a little dilated. In 232 injections containing one to five milligrammes the pupil was never contracted as it is after the direct instillation of eserine into the eye.

There is an enormous difference, as Dr. Bouchut has proved, between the effects of eserine in man and in the lower animals, the greater part of the symptoms observed in the latter not being developed in man, and, on other the hand, those which are observed in man are not found to exist in the brute creation.

With reference to the practical question as to the action of eserine in the treatment of chorea and convulsive diseases, Dr. Bouchut shows that in the dose of three milligrammes in injection, and five to six milligrammes taken by the stomach, the alkaloid arrests or moderates the choreic movements during the period of its elimination. This effect is only temporary, but still, in proportion to the diminution of the movements in question, the disease itself is relieved and finally disappears. The particulars of twenty-four cases are given by the author, and he shows that of the cases treated by injection the successful result was obtained by an average of seven injections, and, taking

the results of injection and administration by the mouth together, the average duration of treatment was ten days.

The general conclusions drawn by Dr. Bouchut from his observations are the following:—Eserine and its sulphate may be employed either in hypodermic injections or by the stomach, and they should be administered to the patient fasting; the action is most energetic when injections are used. The action of the drug lasts from one to three hours, and then ceases entirely, so that the dose may be resumed to the amount of fifteen to twenty milligrammes given at intervals during the day. Eserine usually causes paleness of the face and contraction of the pulse sometimes followed by retardation, and almost all the patients experimented upon had uneasy feelings, epigastric pain, nausea, and spitting of stringy watery fluid, and sometimes there was bilious vomiting. The alkaloid does not sensibly alter the temperature, and in the doses above described it does not cause colic or diarrhœa; and when given internally it leaves the pupil in its natural state of contractility, the dilatation or contraction of this aperture being only exceptional. When administered in cases of chorea the disease is gradually cured in about ten days, and the method by injection is more certain in its effect than the administration by the stomach. Dr. Bouchut has never seen tremblings or convulsions caused by eserine, and he thinks it probable that these effects are only produced by large and poisonous doses.—*Bulletin Général de Thérapeutique*, April 15th, 1875.

On the Treatment of Malignant Pustule by Phenic (Carbolic) Acid. By Dr. Estradère, of Bagnères de Luchon.—For the last three years Dr. Estradère has treated cases of malignant pustule by a method which he believes to be entirely novel, the chief remedy employed being phenic acid used internally as well as externally. The cases he gives are eight in number, the disease having been contracted in various ways, sometimes by the contact of animals which had died of malignant pustule, sometimes by the bites of insects. The first case was a fatal one, being caused by stripping a heifer, and it is adduced to show the inutility of the ordinary treatment by cauterizations of nitrate of silver and hot iron, and the internal use of ammonia, quinine, &c. The fifth case, which the author considers the most striking one, was that of a butcher who had stripped two cows which had died of malignant pustule. The seat of the patient's disease was the neck, and Dr. Estradère made a crucial incision of the pustule, and afterwards applied the nitrate of silver. The disease, however, became worse, and the œdema spread over the scalp, the forehead, the lips, and the chin, besides the neck and the upper part of the chest. The place of the original pustule was converted into a black eschar, and two large incisions were made over the pectoral muscles; at the same time the phenic acid was prescribed internally, and compresses of the same substance were applied over the neck and over the incisions. From this point of the treatment the patient gradually improved; the eschar was at the end of a fortnight separated and removed, the subjacent part became of a bright red colour, granulations sprang up, and cica-

trization was completely effected. Reviewing the history of his eight cases, the author shows that of two of them, treated in the ordinary way, one died and the other recovered with difficulty, but in the cases treated by the phenic acid the beneficial effects were observed in all and the treatment was uniformly successful. Dr. Estradère thinks he has established the fact that the phenic acid, used internally and externally, is the best remedy for malignant pustule and that all other treatment may be abandoned.—*Bulletin Général de Thérapeutique*, June 15th, 1875.

On the Use of Nitrite of Amyl in various forms of Spasm, and on its Value as an aid to Diagnosis. By Dr. S. Weir Mitchell, of Philadelphia.—Dr. Mitchell has for some time entertained the conviction that nitrite of amyl would be a suitable remedy in epilepsy, because it rapidly induces fulness of the vessels of the whole head, and thus would counteract the condition of vascular spasm which characterises the outset of the epileptic attack. The cases, however, are rare in which the remedy can be employed, because the fit comes on so suddenly as to prevent the due administration of the nitrite, but in certain instances the patient has a succession of fits within a limited space of time, and, being then in bed, is so placed as to admit of the trial of this plan. The first case in which Dr. Mitchell was able to test the value of the remedy occurred in 1872, and the result was quite successful. The case was one in which the disease was caused by sexual abuse, and various remedies had been employed in vain, but, as a last resource, Dr. Mitchell gave the patient three or four drops of nitrite of amyl, and directed him to inhale it by putting the open phial which contained it up one nostril while he closed the other nostril and then made a few full inspirations. At the second trial of the experiment the patient felt his face flush, the carotids beat violently, the head felt full, and the spasm being thus caused to cease, the impending attack was cut short for the first time in the course of the epileptic seizures. On subsequent occasions the attacks were arrested in a similar manner, and Dr. Mitchell reports that for the last two years and a half there have been only seven fits, or rather only one, for all the rest have been cut short by the nitrite. Several other cases are given in which the results were the same, and although Dr. Mitchell does not allege that the drug has any power to prevent the return of the fits, he has no doubt of its efficacy in arresting the actual convulsion. In reference to the aid given to diagnosis by the use of the nitrite, Dr. Mitchell writes with some hesitation, but he thinks that in some doubtful cases of cerebral disease the nitrite may help to clear up the difficulty. When, for instance, the malady is truly epileptic, the nitrite of amyl may arrest the fit in the manner already described; but when the disorder is of the congestive type, the drug may prove useful in settling the question of its nature by reproducing the train of symptoms and thus showing its real character.—*Philadelphia Medical Times*, March 6th, 1875.

On the Therapeutical Effects of Dietetic Treatment. By Dr. Dauvergne, Sen., Physician of the Manosque Hospital.—After quoting a

sentence formerly written by Prof. Bouchardat to the effect "that young physicians, as they advance in life, will see, as he did, that drug medication will not keep all its promises, and that they will often resort to the careful use of hygienic measures in the treatment of disease," Dr. Dauvergne signifies his concurrence in the view expressed, and proceeds to give several cases in illustration of its truth. The first case was that of a lady who had a swelling of an uncertain character in the right hypochondrium, attended with the most alarming symptoms and acute pain, but which, after resisting the employment of poultices of opium and belladonna, yielded to the application of tepid water on a napkin folded and laid over the painful region. Another case was that of a boy twelve years old, who had an induration of the whole abdominal region, with hectic fever, but who, after trying various remedies, recovered entirely by being put upon an exclusive diet of white grapes. A third instance was afforded by a man at the Manosque Hospital with an engorgement of the liver, with jaundice, but who was eventually cured entirely by being placed upon a diet consisting of water-melons, white grapes, figs, and peaches, without bread or soup. Dr. Dauvergne states that he has also cured several cases of anasarca and ascites by means of a milk diet; and he adduces a striking case in which an old gentleman, nearly eighty years old, and who was swollen all over his body and was daily expected to die, recovered under the use of milk and onion soup. But it should be added that purgatives and digitalis were also employed at the same time. While adducing these examples of the good effects often attending a judicious diet, Dr. Dauvergne notices other cases where maladies have been aggravated and even death caused by erroneous diet. This latter class of cases chiefly consisted of fevers, in which the malady appeared to be going on favorably, but in which the symptoms reappeared under injudicious alimentation. Dr. Dauvergne's observations, although by no means new, are well worthy of attention, and his cases afford very good illustrations of the views he advances.—*Bulletin Général de Thérapeutique*, May 30th, 1875.

On the Use of Cold Baths in Cerebral Rheumatism.—At a recent meeting of the Société des Hôpitaux in Paris the use of cold baths in cerebral rheumatism was the subject of discussion, M. Féréol introducing to the members an account of a case so treated. The patient was thirty-four years old, of quiet and temperate habits, who was suffering from acute articular rheumatism. He was treated at first with emetics, sulphate of quinine, and colchicum, but in five days he was seized with delirium, agitation, and dyspnœa, and at the same time the pains in the joints disappeared. The temperature of the body rose to forty degrees (Centigrade), and leeches, calomel, and bromide of potassium were given without success. The temperature rose further to forty-one degrees, and blisters were placed on the hairy scalp and digitalis was given. There was then a little more rest, but the aspect was typhous, with stupor and continuous sub-delirium; sleeplessness, agitation of the muscles, subsultus tendinum, dry tongue, &c. After some consultation with other physicians it was determined

to try the effect of cold baths as the only remaining resource. This plan was pursued for a whole week, the patient remaining under close observation the whole of the time, and the thermometer being almost fixed under the axilla. As soon as the temperature rose to 39·5° the patient was plunged into a cold bath. From the 25th of February to the 3rd of March sixteen baths were administered at a temperature varying from twenty-one to twenty-five degrees (Centigrade), and the duration of each bath was twenty minutes on the average. The patient always raised the temperature of the water from one to two degrees, and, on leaving the bath, his own temperature fell to thirty-six degrees. After several fluctuations and much anxiety on the part of the medical attendants, the patient eventually recovered completely. M. Féréol insists on the absolute necessity of constant and intelligent supervision during this mode of treatment, and he states that the life of the patient depends on such care being unintermittingly bestowed. This was the third case of cerebral rheumatism cured in France by the use of cold baths. Dr. Dujardin-Beaumetz, in making some observations on M. Féréol's case, thought that it was necessary, before laying down precise rules as to the treatment of cerebral rheumatism by cold baths, to determine what was meant by the disease in question. He believed that the treatment was inapplicable to many of the cases of cerebral rheumatism so called, and that it was only adapted to that form of disease described by Trousseau as *névrose rhumatismale*, and by Wunderlich as *maladie rhumatoide à forme nerveuse.—Bulletin Général de Thérapeutique*, March 30th, 1875.

On the Action of Various Drugs on the Biliary Secretion. By Prof. Rutherford and M. Vignal.—At the recent meeting of the British Medical Association at Edinburgh, a report was made by Prof. Rutherford on a series of experiments made upon dogs to ascertain the effect of various drugs in promoting the secretion of bile. It had been shown by Prof. Hughes Bennett's committee that, in dogs with permanent biliary fistulæ and living on a fixed diet, "spontaneous diarrhœa, dysentery, and purgation, produced by blue pill, calomel, corrosive sublimate, and podophyllin, always diminished the solid constituents of the bile, and, with one exception, the fluid portions of the bile also." More recently, Röhrig performed experiments on the action of cholagogues in fasting curarized animals with temporary fistulæ, and found that large doses of croton oil greatly increased the secretion of bile, and that a similar effect, though to a less extent, was produced by colocynth, jalap, aloes, rhubarb, senna, and sulphate of magnesia, the relative power of producing hepatic stimulation being in the order just mentioned. Prof. Rutherford and M. Vignal have performed a further series of experiments, adopting Röhrig's method with some modifications. Their conclusions in some respects confirm and in others modify those of the last-named observer. Croton oil, although violently irritating to the alimentary canal, is shown by them to have but little action on the liver. Podophyllin was proved to increase greatly the biliary secretion, and aloes was also shown to be a powerful hepatic stimulant. Rhubarb was proved to be a more important hepatic stimulant

than Röhrig had stated it to be; the cholagogue effect of senna is less than that of rhubarb, but colchicum is a very decided cholagogue, taraxacum is a cholagogue, but not a powerful one, and scammony has a slight cholagogue action. In four experiments with calomel, the secretion of bile was slightly increased in one, but there was nothing but diminution of the secretion in the other three; purgative action, however, was produced in all, and the bile was rendered more watery. The experiments on which these statements are founded are shortly to be published *in extenso*, and the above is only a brief abstract of the *résumé* of Prof. Rutherford's report. — *British Medical Journal*, Aug. 14th, 1875.

On the Uses and Administration of Phosphorus. By Dr. Kirby (Pamphlet).—Dr. Kirby, like most other writers on the subject, regards phosphorus as a medicine especially calculated to supply or to restore phosphorus to the system, and he specifies the cases in which its use is indicated. But, as is well known, this element is now employed in many maladies besides those in which its chemical action is specifically required, and including, for instance, neuralgia, hysteria, melancholia, epilepsy, &c. The mode of administering phosphorus, however, so as to secure its beneficial operation and to avoid its poisonous action, has long been and perhaps still is a question requiring a definite solution. The 'Additions to the British Pharmacopœia' made last year, give, as is well known, two directions for preparing phosphorus for medical use, namely, the "Oleum Phosphoratum" and the "Pilula Phosphori," while Dr. Ashburton Thompson, in a work lately published by him, recommends the use of phosphorus dissolved in cod-liver oil, or combined with zinc, and others have recommended the use of the phosphites and hypophosphites of soda, potash, and lime, as a means of introducing phosphorus into the system. But Dr. Kirby maintains that the successful employment of phosphorus depends entirely on its administration in the free state, that is, before its conversion into phosphoric acid or other phosphorus compound. With this object in view he thinks it essential that it should not be exposed to oxidation, nor the digestive functions disturbed or disordered by it or by the menstruum or vehicle in which it is administered. He therefore recommends the use of phosphorus in a pill, which he calls Pilula Phosphori Mollis, because it is a soft and soluble pill-mass, and to distinguish it from the Pilula Phosphori of the British Pharmacopœia, which is hard and insoluble. The pills recommended by Dr. Kirby contain one grain of phosphorus in fifty, and therefore five grains contain one tenth of a grain of free phosphorus. Five grains is the maximum dose, and half a grain, which contains one hundredth of free phosphorus, is the minimum.

On the Action of Salicylic Acid.—Dr. Winter, in a recent number of 'Schmidt's Jahrbücher,' in recording the different results obtained by the use of salicylic acid, remarks, in the first place, that this acid may completely replace carbolic acid as a disinfectant in recent and chronic ulcers when applied on the bandages. In several cases of recent superficial gangrenous sores Dr. Wagner applied a thin layer of pow-

dered salicylic acid on the surface and placed over it some wadding. Sometimes the secretions of the sore passed through the bandage, which, however, was inodorous, and then another layer of wadding sprinkled with salicylic acid was laid over it. For the most part the bandage might be removed in a week, and the healing of the sore was accomplished; and Dr. Wagner, without denying the efficacy of the wadded bandage, attributes a great part of the successful result to the disinfecting properties of the salicylic acid. In atonic ulcers of the foot an obvious acceleration of the granulating process was effected by a salve of salicylic acid and lard. Dr. Wagner has also employed this agent successfully in the form of gargle in ulcers of the gums, stomatitis, &c., and the foul smell from the mouth has been at the same time corrected. The same authority recommends the use of salicylic acid in all maladies which take their origin from minute organisms. In diphtheria the acid seems to display great efficacy and to shorten the duration of the disease very materially, and it may be given internally and also be used as a gargle. Dr. Karl Fontheim has likewise employed salicylic acid successfully in diphtheria, using the remedy both internally and as a gargle.

[Salicylic acid is so called because it was originally obtained by the action of potash, aided by heat, upon salicine, the bitter principle of the willow-bark, the alkali being neutralised by hydrochloric acid and the salicylic acid precipitated. But Prof. Kolbe has lately invented a new method of preparing salicylic acid by the action of carbonic acid, aided by heat, on a solution of phenol in caustic soda.—REPORTER.]—*Schmidt's Jahrbücher der Gesammten Medicin*, June 17th, 1875.

On the Internal Employment of Sea-water. By Dr. LISLE, of Arcachon.—Dr. Lisle was induced to try the effect of salt water on himself in consequence of hearing that some sailors at Marseilles, being in bad condition from the want of fresh water, had taken it into their heads to make some bread with salt water, and had found their strength, which had been exhausted by privation, restored by eating it. He caused some bread of this kind to be made by a baker of the country, who, after some unsuccessful trials, managed to produce an article which was more palatable than ordinary bread. Dr. Lisle ate this bread himself, and indeed tasted no other for more than eight months, and experienced the greatest possible benefit from the change. He had previously suffered for nearly thirty years from a nervous affection of the stomach, of a somewhat indefinite kind, of variable duration, frequently accompanied by severe disturbance of the digestive functions and more or less complete loss of appetite. Less than a fortnight of the new system sufficed to restore the appetite, to regulate the digestive functions, to improve the nutrition, and to induce deep sleep exempt from the nightmare and the frightful dreams by which it was formerly disturbed. He then recommended the sea-water bread to all persons, whether ill or well, who consulted him; and while none of them suffered any ill effects, they all declared that it was more agreeable to the taste, kept fresh a longer time, and was more easily and rapidly digested than common bread. From the results of

Dr. Lisle's own observations and those of a medical colleague at Arcachon, he was able to arrive at the conclusions that the sea-water bread restores and increases the appetite, renders digestion more rapid and easy, and actively stimulates all the nutritive functions, and that it is the best plan to use it in order to maintain the elements of the blood in their normal proportions and to reconstitute this fluid when it is impoverished. The experiment of using this sea-water bread, then, was quite successful; but as there are practical difficulties in making it away from the sea, Dr. Lisle went a step further and endeavoured to find some other method or methods of administering the sea-water, and, therefore, in addition to the bread, for the preparation and baking of which he gives due instructions, he recommends the use of a *syrup* of sea-water, made by mixing the water with sugar, and an *elixir* made with rum and sugar. Dr. Lisle does not claim for salt-water any specific property in the cure of diseases such as is possessed by quinine or mercury, but regards it as a simple hygienic agent, useful only in an indirect manner by its general operation on the blood and the nutritive functions. The sea-water bread and the *thalassic* syrup and elixir (so he names them) appear to him to be indicated as hygienic and preservative agents against disease in persons who are healthy but of delicate constitution; in convalescence from acute diseases; in all apyretic derangements of the stomach and of the digestic functions; in the neuropathic disturbances accompanying impoverishment of the blood, such as anæmia, chlorosis, hypochondriasis, asthenic insanity, &c.; in the preservative and curative treatment of most of the morbid diatheses, and especially the scrofulous and tuberculous; and in diabetes in all the phases of its development. Dr. Lisle claims for sea-water all the properties of a mineral water, and he gives in a table the analyses of specimens of water taken from the sea at Arcachon, compared with those obtained from Nauheim, Kreuznach, Hombourg, Soden, and Balaruc, showing the abundance in sea-water of the principles to which the usual mineral waters owe their efficacy. The mineralisation of sea-water is, in fact, much superior to that of any other water of the kind, and thus it will be found, as Dr. Lisle hopes, to possess at least all the hygienic and therapeutical properties of the class of which it may be considered the most perfect type.—*Bulletin Général de Thérapeutique*, Feb. 15th, 1875.

On the Hydrotherapeutic Treatment of Intermittent Fevers. By Shirley Deakin, of Calcutta.—After offering a few observations on the treatment of continued fevers and other affections of a similar nature by cold water, Mr. Deakin gives the results of his own experience in the treatment of intermittent and remittent fevers by this method in the Hastings Coolie Hospital, Calcutta. The cases recorded are ten in number, and they are arranged in a tabular form, giving the temperature before and after the bath, three times a day, for a varying period, the longest being eight days. The patients were mostly bathed in Mr. Deakin's presence, and the time of immersion was taken by watch. The ten cases given in the table were part of twenty cases, which included nearly all those of fever admitted during three weeks

into the hospital. Some of them were mild cases, but in one of them the temperature rose to 104° F., in another it twice reached 105° F., in a third it reached 104·6°, and in a fourth it reached 106° on one occasion only. During treatment the only drug given, except a dose of pulv. jal. co. on admission, was some camphor water as a placebo. The temperature was invariably reduced after the bath, although the mouth temperature, as taken by the thermometer, did not by any means represent the amount of heat abstracted. The patients all recovered. As a rule they liked the baths, and were grateful for a cool instead of a burning skin. In none of the cases was there any enlargement of the spleen or liver remaining after treatment. The baths were given three times daily, at 6 a.m., 1 p.m., and 6 p.m. After being kept in the bath half an hour, or for twenty minutes only if the temperature was normal, the patient was ordered to walk about, but, if not strong enough, he was at once wrapped up in a warm blanket. The temperatures were taken just before immersion and about half an hour after the bath. Mr. Deakin recommends small doses of quinine in addition to the baths; gr. ij of quinine being then as efficacious as twenty or thirty without the bath.—*Indian Medical Gazette*, Nov. 2nd, 1874.

On the Relative Strength of Chloroform and Ether, and on their Use as Anæsthetics. By Dr. Oscar H. Allis, of Philadelphia.—Dr. Allis was first entrusted with the administration of chloroform in the surgical clinical department of Jefferson Medical College in the year 1867, and in that capacity assisted the Professor of Surgery for more than two years. He has devised some simple forms of inhaling apparatus, which he figures and describes; but he remarks "that safety in the administration of chloroform does not lie in an inhaler, but in him who uses it." He thinks that it is far safer to produce anæsthesia by an equal and constant supply of chloroform, namely, drop by drop, than by pouring it on at intervals, even in quantities so small as half a drachm. When he uses it he drops the anæsthetic from a graduated bottle containing three drachms, a drachm and a quarter of which is the quantity necessary to produce complete anæsthesia. His apparatus for inhaling ether is different from that for chloroform, the former requiring a larger evaporating surface, by which a more rapid evaporation is produced and consequently a more rapid anæsthesia, and there is also a greater economy of ether. But he also describes an extemporaneous inhaler, which may be constructed in a conical shape from a towel and a newspaper. From his practical experience Dr. Allis lays down certain rules for the use of anæsthetics, among which some of the most important are that the stomach should be nearly or entirely empty, and that the reclining posture should always be adopted in administering chloroform. As to the relative strength of chloroform and ether, he has found by experience, in giving chloroform and ether to five persons under similar circumstances, that the former is four-sixth times stronger than the latter; and in comparing ten cases, as nearly alike as possible in age, sex, and strength, he obtained a similar result. He considers chloroform far more dangerous than ether when

used as an anæsthetic, but a combination of ether and chloroform has been found to be less fatal than the latter used alone. In the use of ether he states that safety may be guaranteed if ordinary care be taken while chloroform has caused many sudden deaths. Part of the mortality from the latter he attributes, however, to want of care on the part of those who administer it.—*Philadelphia Medical Times*, December 5th, 1874.

On the Successful Use of Jaborandi in Diabetes Insipidus or Polydipsia. By Dr. Laycock, of Edinburgh.—Dr. Laycock points out that both forms of diabetes as well as certain kinds of Bright's disease are really *neuroses*, having their seat in that part of the encephalon which regulates the amount of water in the blood, and has therefore both anatomical and functional relations with the sudoriparous glands and the kidneys, and with the appetite for water and the sense for thirst. He now relates two cases of diabetes insipidus or polydipsia in which jaborandi was given with good effect. In the first case the quantity of urine passed was very great, amounting to 400 and 500 ounces per diem, and the patient was compelled sometimes to micturate every half hour. The urine was pale, almost colourless, faintly acid, of sp. gr. 1005, with no sugar or albumen, and a very small amount of the ordinary solids. He was under observation and treatment from Dec. 24th, 1874, to Feb. 26th, 1875, when there was still great thirst and the skin was dry, and the daily amount of urine voided was 300 ounces. Jaborandi was now ordered in the form of an infusion (one drachm of the leaves and twigs to six ounces of water), and a dessert-spoonful was taken every four hours, this dose being increased and given at shorter intervals on succeeding days. On March 5th, or about a week after the jaborandi was given, the skin of the back, abdomen, and inner aspect of the thighs was found to be perspiring pretty freely, and on the 6th the skin of the arms and left palm perspired. On the 15th the quantity of urine had declined steadily from 300 ounces to 236; and on the 31st, the jaborandi treatment having been still continued, the quantity fell to 180 ounces. The urine continued to diminish in amount till the middle of May, when it amounted to 120 ounces a day, and the patient was then discharged at his own request. In the second case the daily quantity of urine passed was 128 ounces, the sp. gr. being 1008, of acid reaction, and containing some albumen, but no sugar. There was great thirst, and in order to quench it the patient was obliged to drink a large quantity of water at a time. This patient was placed at once on the jaborandi treatment, one table-spoonful of the decoction being given thrice daily. The quantity of urine passed and of fluid drunk in twenty-four hours was carefully noted, and the results were that the amount of urine fell in about two months to ninety-eight ounces, and that of fluid drunk fell to 100 ounces, the amount at first being 186 ounces.—*Lancet*, Aug. 14th, 1875.

On the Use of Iodide of Potassium in Syphilis. By Dr. Joseph R. Beck, of Indiana.—In this paper, which is, however, far too dogmatic in its general tone, Dr. Beck declares himself an opponent of the use

of mercury in any form in the treatment of syphilis, although he employs that mineral in other diseases. He admits that secondary symptoms are dissipated rapidly under the use of mercurials, but he argues that in such cases the disease is only masked, and that tertiary manifestations are sure to ensue. He goes on to make the following strong assertion, namely, "*that every case of secondary syphilis which has been successfully (?) treated by mercurials will, as surely as the sun rises, reappear as tertiary syphilis, if the patient lives long enough.*" (The italics, &c., are the author's.) Dr. Beck's treatment, which he explains at length, consists in giving large and increasing doses of the iodide of potassium, in combination, however, with iron; and he also gives Fowler's solution of arsenic at the same time, and if anæmia be present, which he says is frequently the case, then he adds quinine, gentian, cod-liver oil, and valerian. If any ulcers be present, they are dressed with hydrate of chloral dissolved in distilled water. This constitutes the first step of the treatment, but the subsequent prescriptions increase the doses of the iodide of potassium and of the Fowler's solution, until iodism is established, when the use of the medicine is stopped for a week; then it is recommenced, and if iodism again ensues, Dr. Beck says that he "confidently discharges the patient, perfectly and permanently cured, with the poison of syphilis and that of mercury for ever eradicated from the system." In order to prove the truth of the last assertion, he tests every patient, about a year after all treatment has ceased, with ten grain doses of the iodide of potassium, and the test has invariably produced profuse iodism before one drachm of the drug has been taken. The presence of iodism is regarded by Dr. Beck as the certain evidence of permanent cure. The iodide of sodium has been in Dr. Beck's hands negative in its results.—*Philadelphia Medical Times*, March 13th, 1875.

REPORT ON PATHOLOGY AND THE PRACTICE OF MEDICINE.

By John T. Arlidge, M.D., A.B. Lond., F.R.C.P. Lond.,

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The Pathology of Sunstroke.—Dr. Rudolph Arndt gives the history of three cases of sunstroke occurring in the persons of three healthy young soldiers after a long and fatiguing march, and follows the account by a review of the pathological changes met with after death.

There are two features standing in strong contrast—the blanching of all the organs, and, therewith, the over-distended condition of all their vessels, above a certain size with dark-coloured, uncoagulated blood. The skin and muscular tissue were bloodless, but their large vessels full to bursting, exuding large drops of blood when wounded.

The brain was in the same anæmic state together with its membranes, whilst the large veins and the sinuses were distended with dark, unclotted blood. The same condition obtained in the heart, pericardium, liver, and kidneys, as well as in the mucous membrane of the intestines and bladder. In consequence of their extreme distension, the blood-vessels were much increased in size, and in some places, as beneath the endocardium, the pericardium, and both pleuræ, the overstretching had led to ecchymoses.

The singularly bloodless condition of the brain-substance is an appearance contradictory to the generally received opinion that sunstroke is hyperæmia of the brain. This notion must have arisen from imperfect observation, noting the extreme engorgement of the larger vessels, especially of the veins, and confounding the escape of blood from such vessels, and the staining of the tissue thereby, with exudation from the capillary and smallest vessels of the viscus. But the fact is, the capillaries and minute vessels are well-nigh empty, and in cases more fully developed completely so, and their walls collapsed.

The cause of the parenchymatous anæmia is apparent. In all three cases the brain was swollen. It distended the sac of the dura mater; the gyri of the hemispheres were widened, flattened and pressed together, obliterating the interspaces. In two of the three cases in which the abdominal viscera were examined the liver and kidneys exhibited the same enlargement, from simple swelling of their mass. The liver had acquired a more rounded form than normal, and its borders were thicker. Its transverse diameter seemed most enlarged. It had a doughy feeling, and was readily impressed by the fingers, the marks remaining. Its acini were distinct. The enlarged kidneys allowed of the ready separation of the capsule. The pale cortical substance looked swollen and widened, whereas the medullary substance was injected with blood, the congestion being greater as the pelvis was approached, this last part exhibiting great injection of its vessels with ecchymoses.

The swollen brain was likewise unusually wet; in two cases there was an excess of serum in the ventricles, and in the third, if not actual excess, a considerable proportion. On slicing it a watery fluid escaped, showing its highly œdematous condition. Its membranes, which were in places raised in a bladder-like fashion, were readily separable from the subjacent tissue.

A similar, if not identical, state of œdema existed in the kidneys and liver. In the latter, indeed, a dryness was remarked, but the distinctness of its acini indicated the presence of some interpenetrating matter, which might well seem to be nothing else than the serum of the blood. The greater moisture of ordinary liver-tissue is explicable from the fact of the presence of blood throughout it, whereas in the liver of those dead from sunstroke the hepatic tissue itself is bloodless.

In the several organs the capillaries and smallest blood-vessels

were compressed, and the blood forced out of them into the neighbouring veins.

The heart was contracted in all the cases; in one the left side was more so than the right. It felt hard, and its naturally bright colour was replaced by a dusky red or greyish brown. The tissue was dry and fragile, and thin sections of it had a lustrous aspect. The same phenomena were present in the muscles generally.

The dry condition of the heart-substance and of the muscular tissue at large is one contrasting with the œdematous state of the brain-matter; and Arndt, after remarking this fact, and taking with it the dry, though swollen, state of the parenchyma of the liver, and, though in a less degree, the like condition of the cortical substance of the kidney, is inclined to attribute that state to something else than simple œdema, and supposes an overgrowth in size of the constituent elements of the tissues, and an excessive formation of protoplasm over-rich in granules. After further discussion he comes to the conclusion that the essential nature of the process—as best illustrated in the heart-tissue and muscles—is inflammatory. Although wanting in the usually recorded characters of encephalitis, he nevertheless considers the lesion met with in the brain to be of the nature of a parenchymatous inflammation, the process being modified by the peculiar circumstances of the attack and by the special nature of the tissue.

The history and phenomena of sunstroke are very fully entered upon by Arndt, as seen in various stages and degrees of the malady. He particularly insists on the very high temperature of the body accompanying it, and on the consequent interference with all the vital functions, particularly with the destructive metamorphosis of tissues, and the elimination of used-up material. He points out that the blood is loaded with excreted material, that it has a black colour, does not coagulate, and is rich in carbonate of ammonia. The lungs are congested in their dependent parts, and the bronchial mucous membrane intensely injected and swollen. In the greatly elevated temperature, and the many consequences dependent upon it, Arndt finds an analogy with the most marked septic diseases.

Hydrophobia treated by Chloral.—Dr. V. Grazi records a case of hydrophobia under his care in the Hospital of Santa Maria Nuova, Florence, in which large doses of chloral were given ineffectually. The patient was a woman, æt. 52, and was bitten by a dog of her own on the nose. Her husband, a child, and a servant of the hospital were subsequently bitten in the hand by the same dog, but it is not noted that any of these suffered. The period of incubation was fifty days, during which the woman appeared in perfect health. Her admission took place on the 27th of November. On the 25th her appetite failed, and she felt constriction of the œsophagus in drinking water, and an aversion to drink. The following morning she ate a good breakfast, but drank nothing; at dinner-time she could not eat, and painful constriction occurred on an attempt to drink. When admitted her mind was perfectly clear and she gave a precise account of her accident and symptoms.

Immediately after her admission at noon a gramme (15 grains) of chloral was administered. A consultation was held, and it was resolved to give chloral in large doses. Accordingly three grammes were at once prescribed, and small pieces of ice ordered to be frequently swallowed. In the course of the 28th she took four grammes. On the 29th, in the morning, three grammes were given by clyster and four by the mouth; in the evening of the same day the like doses were repeated in the same fashion. The same plan was pursued on the following day, when the œsophageal pain disappeared and she became tranquil in mind; but on the 1st of December the pharyngeal constriction had increased, although the drug had been persevered with. The pulse was weak, irregular, and 120, respiration very frequent: the chloral was reduced to four grammes. The day was passed tranquilly, but the signs of sinking continued, accompanied by some slight involuntary muscular contractions, and at eleven at night, after speaking to a nurse, she suddenly expired.

The body was examined thirty-five hours after death. The cadaveric rigidity was very great, particularly in the upper limbs and face. On opening the cranium the meninges were found much injected with black blood. A clot of blood existed under the arachnoid over the pons Varolii, and to this is ascribed the sudden death. The spinal meninges were somewhat injected, especially the pia mater. The cord and the spinal nerves exhibited no change in colour or consistence. The vessels generally were filled,—the veins the more so, with intensely dark blood, diffluent and nowhere coagulated, and not changed in colour by exposure to the air. Nothing abnormal was found under the tongue. The pharynx was lined by a false membrane extending downwards to the œsophagus, attributable to the large doses of chloral swallowed, which, though enclosed in crumb of bread, escaped more or less by reason of the difficulty of deglutition and came into direct contact with the mucous membrane. The lining membrane of the larynx preserved its normal colour as low down as the ventricles, but below this point was congested; the vascular injection being more intense towards the bifurcation into the two primary bronchi, where a tenacious mucus covered the congested surface. The lungs were gorged, particularly behind and below. The heart was hard and contracted, its tissue of normal colour: little blood existed in its cavities. The liver, excepting being gorged with blood, appeared healthy. The gall-bladder contained some bile of ordinary character. The spleen was hard and small. The kidneys were slightly enlarged, hard and hyperæmic. The stomach was contracted, and devoid of alimentary matters; its membrane had at some points a rosy hue and was besmeared by abundant mucus. The intestines contained very little fæcal matter; the mucous membrane showed some rosy patches, but no structural alterations were visible.

Dr. Grazi observes that he has narrated this case, not because of any special features presented, but to induce others to give chloral in large doses, because of the relief thereby afforded to the terribly painful symptoms of the malady. For several days together he

administered, in the course of twenty-four hours, as much as fourteen grammes of that drug, without misgivings, and much to the relief of the sufferer, who swallowed it willingly, notwithstanding the pain accompanying the act; and he considers the convulsions, the painful muscular contractions, the constriction of the gullet, the mental disturbance and delirium, the propensity to injure others, and all the other symptoms of this most terrible disease, were greatly lessened in intensity. As to the pathology of the malady Dr. Grazi has no opinion to offer.—*Lo Sperimentale*, August, 1875.

Notes on Tetanus.—E. Hansen reports three cases of tetanus, exhibiting similar features, but of as many different forms of origin. The first case is called one of rheumatic tetanus, no other cause than exposure to cold and a chill being known. The second is an example of traumatic tetanus, following a wound of a finger, the wound remaining open at the time the tetanus appeared. The third case was that of a girl, æt. 10, and probably cannot be rightly classed with tetanus, as the symptoms seemed attributable to spinal meningitis or myelitis, for together with tetanic contractions there were violent vertebral pain and tenderness on pressure. Nevertheless no fever presented itself, the pupils were not contracted, and paralysis never appeared.

In each case the symptoms followed generally this order: muscular stiffness of the neck, trismus, then violent epigastric pains due to diaphragmatic contractions, considerable difficulty of respiration, and general tetanic contraction, excepting in the arms, which in all instances remained free. In the first case the general contractions curved the body forward; in the second, backward; and in the third, to one side. In the last it is worth noting that the pleurosthotonos, having previously been on the right side, became transferred to the left several hours before death. This circumstance indicates the involuntary nature of the curved position assumed, and that the position is not of the nature of a voluntary act intended to give relief.

The two former cases got well, the recovery being attributed to morphia injections. In the first named these injections were supplemented by the internal exhibition of chloral, the morphia seeming not sufficient of itself to induce calm. In the case of the child the morphia injections also acted well, though ineffectual in saving life. Injections of curare (woorara) were not only powerless for good, but actually augmented the pain and tetanic paroxysms. Another noteworthy fact is that, with the two adult patients, passive movements did not provoke convulsions, whilst voluntary motions invariably did so. (*Dorpat. Medicin-Zeitung*, Band v, p. 230, and *Révue des Sciences Médicales*, July, 1875.)

Section of Nerves in Neuralgia.—MM. Arloing and Tripier contributed a memoir on the division of nerves in neuralgia to the medical section of the French Association. The authors consider that the phenomena of recurrent sensibility have not been sufficiently kept in view either in human pathology or therapeutics. By experiment they have satisfied themselves that the property of recurrent sensibility is partaken by nerves at large.

It is well known that it is rare in neuralgia, especially the functional varieties, to find the whole of a nerve-trunk the seat of pain; were it otherwise, no spot on the surface that received fibres from any trunk would be free from pain. As it is, of the several constituent fibres only some are affected. Moreover, neuralgia does not occupy the whole course of nerves or of their branches. The pain is commonly confined to certain limited spots, pressure upon which will at once induce it. As Valleix first pointed out, it is the most superficial nerves that are principally affected, and that their points of emergence on the surface are the chief seats of pain. The trifacial and thoracic nerves constitute the greatest exceptions to these rules. The writers consider that no adequate explanation of the above-named facts has been offered. By the very simple experiment of pressing the ulnar nerve at the elbows, they say, two sorts of pain are shown to exist. If gentle pressure be made, the pain is felt exactly at the level of the point pressed upon; but if the pressure be more severe, the pain is felt deep down. This fact stands in relation and harmony with the results they have arrived at experimentally; viz. that all nerves, motor, sensitive, and sensory-motor, possess recurrent fibres; that such recurrent fibres are more numerous on a nerve as it approaches the periphery, and, *per contra*, decrease in numbers and finally disappear altogether at a certain distance along the trunk. They are special adjuncts of peripheral nerve-bundles, and are usually placed immediately beneath the neurilemma. These fibres have necessarily some destination; and as they have never been seen to terminate in the thickness of the nerve-bundles, they may be assumed, from the characters noticed, to end at different points in the tissues adjoining the nerve-trunks or their branches. It is these fibres that transmit to the brain the local sensation of pressure on the ulnar nerve, and that, in certain cases of neuralgia, would be affected by morbific agents; whence an explanation is afforded of the isolated painful spots on the nerve-trunks or on their branches. Another argument in favour of this hypothesis is found in the fact of the diminution, or even the total disappearance, of these fibres above the points indicated by Valleix as the special seat of neuralgic pain, whether spontaneous or provoked by pressure. At the same time it would be an error to assert that these fibres are alone affected in neuralgia; on the contrary, it is probable that they are not always so; yet it would be equally far from the truth to aver that spontaneous neuralgic pains followed the course of the nerves, for usually they break out at several points at one and the same time, and the painful shooting sensations at one moment ascend, at another descend, and will end at times in a single filament. It is the lines of union between these several points that are assumed to represent the course of the nerves. But this assumption is only imaginary. Again, it is supposable that the morbid agency acts at once both on direct and recurrent fibres; consequently, if we examine the mode of extension of neuralgias from a disordered nerve to another hitherto sound, an additional argument in favour of the views propounded is afforded. It is certain that the extension very often takes place by the medium of the

nerve-centres—a fact readily conceivable when it is considered that neighbouring peripheral nerves have commonly also their central fibres in near apposition; but in traumatic neuralgia of a finger it is remarkable to observe that, in place of a neuralgia of the brachial plexus, there is circumscribed pain at the extremity of a collateral and contiguous nerve. The frequent coincidence of trifacial neuralgia with pain of the cervical nerves may be placed in the same category. Lastly, in trifacial neuralgia it is no uncommon thing for the pain to glance from the ophthalmic branch to the mastoid process and the upper portion of the neck. Must it not, therefore, be admitted that the lesion in sensation may be propagated as well by the periphery as by the centres themselves? Still another argument might be gathered from the fact, that in old cases of neuralgia, without appreciable lesion, section of the nerve has completely removed the pain, and that in like manner cases of symptomatic neuralgia have been treated by section of the nerve above the lesion and have relapsed.

Respecting the employment of neurotomy, the writers remark that in neuralgia symptomatic of peripheral lesions, these last must first of all be healed, and that section of the nerve must be of later consideration, for whilst the pain comes and goes the presence of neuritis is probable. In so-called functional neuralgias, likewise, neuritis has also to be borne in mind and dealt with.

We must be guided in our treatment by a consideration of the modifications that supervene on pressure of the painful spot or spots. Thus, if pressure arrests pain on the spot, and, *à fortiori*, if it so do when the compression be made higher up, we are right in concluding that the neuralgia has its seat in both the direct and recurrent nerves, and that section of both of these is necessary to give relief. Where the pain is not stopped by pressure, the inference is, that the lesion is situated exclusively in the recurrent fibres, or else in the nerve nearer the centre, or possibly in the nerve-centre itself. To determine if the pain be in the recurrent branches, pressure must be made over neighbouring nerves to ascertain if the pain be alleviated. Supposing it not to be so, we have reason to suspect mischief higher up in the trunk of the nerve or in the nerve-centre itself. Usually doubt may be cleared away by reference to antecedent and to concomitant conditions, and in certain cases by the use of electricity. As a general rule, in complex cases, it is well to make several associated and peripheral sections, with the object of isolating, so to speak, the mesh of fibres on which the morbific agent is operating. By proceedings of this sort von Graefe has met with great success in dealing with some old and rebellious cases. In the case of a lady at Berlin he made successively seven sections in the orbital region, and effected a cure—a result attributable to the interruption of all the lines of communication with the centre. (*Gazette Hebdomadaire*, August, 1875.)

Papular Erythema related to Rheumatism.—M. Coulard singles out this form of skin eruption from among the several noticed in rheumatism as being pathologically associated with this malady, and

as an indication of a rheumatic diathesis. Admitting the distinction between rheumatic and rheumatoid pains, those accompanying the erythema in question belong to the former. Rheumatoid pains are a feature of hysteria.

True rheumatic pains do not occupy an entire joint, but certain spaces only, which also are especially painful on pressure.

Hardy regards the connection of erythema and rheumatism as accidental; but M. Coulard opposes to this opinion his observation of twenty-one cases, in twelve of which articular rheumatism in an acute form had preceded the eruption, and in the other nine had been manifested also beforehand, but with less intensity.

The eruption, which may be more or less general, is described as occurring in patches of the size of the hand, or of a five-franc piece, separated by narrow strips of healthy skin. These patches are more or less inflamed in the centre, according to the date of their appearance, the oldest being pale in the middle, and surrounded by a wavy erythematous ring of a bright red colour like that of scarlatina. Pressure disperses the colour, but this reappears on its withdrawal.

The eruption is made up of minute red points, uniform in colour, and sometimes accompanied by heat of skin or itching. Some of the rings are confluent, and so give rise to a ribbon-like diffusion. Varieties occur in the shape of the patches, and in their prominence and colour. Usually they are not at all elevated. The colour occasionally is more dusky, and even livid.

The eruption may occur either before, during, or after a rheumatic attack. In the first case its onset is without prodromata, its average duration from five to six days, and its appearance accompanied by heat and itching, but with no fever. In the second case the general phenomena are mixed up with those of the rheumatic fever; but usually some special features are noticeable, such as more heat of skin, dyspnœa, acceleration of pulse, &c. In no case has the eruption been attended by diminution of pain, but rather the reverse. Lastly, where it has appeared after an acute attack there have been more or less severe articular pains. The duration of the eruption is so variable that no rule can be assigned for it. The only general rule (but even this is open to exceptions) is, that the duration is in direct relation to the extent of the rash.

M. Coulard will not venture on the statistics of the frequency of its occurrence in rheumatic cases. He considers that these cannot be gathered from the recorded histories of cases, since those histories are taken with the view of developing facts regarding the ordinary phenomena of rheumatic fever, among which this form of erythema has not been reckoned. Moreover, he will not undertake to decide if this erythema be peculiar to rheumatic subjects, but only go so far as to say that its coexistence with rheumatism is so common, that where it is met with a rheumatic diathesis may be suspected.

It is worthy of notice that many years ago the late Dr. Begbie, of Edinburgh, in his volume of essays, associated erythema nodosum with the rheumatic diathesis (*Reporter*). (*Archives Générales de Médecine*, January, 1875.)

Presence of a Bruit of Fluctuation and Metallic Tinkling in Abdominal Tumours.—M. Laboulbène, Physician of the "Hôpital Necker," records two peculiar cases of abdominal tumour in which a bruit of "hydro-aeric fluctuation" was met with, accompanied with metallic timbre. The *résumé* of the first case is as follows:—A woman, æt. 50, enjoying habitually good health, and the mother of nine children, had noticed of late a swelling on the left side of the abdomen. When admitted, on the 7th of January, 1875, a smooth tumour, without prominences, was found to occupy the left flank in the ovarian region, having the dimensions of an adult head. Pressure caused pain. The skin had its normal appearance. It could be displaced by grasping with the hands without difficulty, but no particular bruit was elicited by brusque movements from right to left. Fluctuation, without being well marked, could be detected; percussion was dull over the tumour, and elsewhere resonant. Fifteen days after, the tumour grew more prominent, and became adherent to the skin, which was now slightly red at the most prominent point. Dull pains were complained of. Percussion now gave a sonorous note, and succussion made brusquely, grasping the tumour between the two hands, produced a noise of fluctuation similar to that caused by shaking a bottle half or three parts full of water. Moreover, on rapidly shaking the tumour three or four times, whilst the ear was applied over it, the noise of fluctuation was noticed to be associated with a metallic timbre. On the 25th of the month Vienna paste was applied to the skin where it seemed thinnest and over the most prominent part of the tumour. In the evening of the next day an opening formed, and a large glassful of purulent, greenish, not thick, discharge escaped, offensive in smell, but not putrid, and accompanied with gas. Great relief followed the escape. The fluid on examination exhibited only the leucocytes of pus.

The treatment consisted in the application of chlorinated compresses, the sac being allowed gradually to empty itself; a drainage tube was also inserted. On the 15th of February the patient was convalescent and sitting up, the sero-purulent discharge through the tube having quite ceased. A weak injection of iodine was introduced into the opening, but it immediately escaped, thereby showing the absence of any sac. On the 23rd the tube fell out spontaneously. On the 20th of March a careful examination showed the existence only of a small, hard tumour, of the size of an egg, and dull on percussion. Pressure upon it caused slight uneasiness. At the end of the month she was discharged cured, and has since continued well.

When first under treatment the opinion of the case was that it was one of ovarian disease, but subsequently it was decided to be a cyst situated between the posterior aspect of the abdominal wall and the coils of intestines, and in close propinquity to the left ovary. The presence of gas within it was attributed, not to any fistulous communication between its interior and the bowels, but to its production by decomposition of the contained purulent liquid.

The second case was that of a woman, æt. 27, who had never had a child and had enjoyed good health. In 1870 she perceived her abdomen enlarge, but during the two following years, as she was free from suffering, she continued her employment, and it was not until the end of 1872 that she entered the hospital (Hôtel-Dieu). Here she was punctured ten times, and on six occasions iodine injections were resorted to after the tapping. The quantity withdrawn varied from one to four litres on the different occasions. According to the patient's account the liquid was always thick, and on the last tapping was of a dirty-grey colour. When admitted into the Hôpital St. Louis (in October, 1874) it was with the desire of having the tumour removed. The tumour occupied the left side of the belly, was movable readily, had an irregular surface, was slightly painful on pressure, and obscurely fluctuated. The skin was not adherent, but was very movable over it. Percussion over the tumour was dull. The uterus was small. The diagnosis was a multilocular ovarian cyst.

Succussion at first produced no special bruit; but some time after her admission the tumour became sonorous, from the presence of gas in its interior, and the hydro-aeric wave, with metallic tinkling, manifested itself. The patient grew weaker day by day, and ultimately died towards the end of November. Unfortunately an autopsy was interdicted, but a trocar was thrust into the tumour and gave vent to excessively fetid gas, to sero-purulent fluid, and to a grumous, white matter.

With respect to the production of gas in closed sacs, M. Hérard had previously noted three remarkable instances of such an occurrence —the first, in a case of empyema and pneumothorax without puncture of the lung; the second, a case of enormous dilatation of the left kidney, which contained pus and gas, and had no communication with the external air; and the third, an ovarian cyst, which suppurated and was occupied with pus and gas without any external communication. In this last case the hydro-aeric fluctuation with metallic tinkling was observed, but it should be noted that the sac had been punctured on three occasions. Demarquay, in his work on "medical pneumatology," refers to other cases.

In conclusion, M. Laboulbène concurs with Hérard that it must be admitted, on the direct evidence of facts, that the noise of succussion in some tumours, and the bruit of "hydro-aeric" fluctuation with metallic tinkling in others, may be perceived in not a few abdominal tumours, and that such phenomena may occur without communication between their interior and the external air.—*Archives Générales de Médecine*, September, 1875.

Splenic Tumour treated by Injection.—Although portions of the spleen and also the entire spleen have been successfully removed when exposed by wounds in the side, it has not so happened when extirpation has been practised for the removal of a diseased spleen. Of nine cases on record the whole proved fatal; consequently the removal of splenic tumours by operation can scarcely be held justifiable.

The success which has attended an injection of iodine in glandular tumours suggested to Professor Mosler the expediency of injecting tumours of the spleen, and to test the practicability of the process he injected iodine into the spleen of several dogs and rabbits. He succeeded well and found the operation to be unattended by intense peritonitis. Subsequently he has had a case of splenic tumour in a woman, into which he injected, not iodine, but a weak solution of carbolic acid in the first instance, and afterwards dilute Fowler's solution. The patient was a married woman, æt. 33, the mother of two children, the youngest being eight years old. Four years before coming under his care she had intermittent fever which lasted a year and a half, and exhibited much irregularity of character. Two years previously she had typhoid fever, and had never been well since, suffering with swellings in various parts of the body. A year before the dropsical swelling extended to the belly, and she became quite invalided by various abdominal disorders. She was admitted into hospital on the 20th of January, 1874. The abdomen was found distended, especially on the left side, where the spleen could be readily felt. Percussion showed it to extend from above downwards fifteen centimètres, and transversely thirteen. The liver was also enlarged, but not to a proportionate extent. No glandular tumours detected; the lungs appeared healthy; there was a cardiac murmur; the urine normal. The blood showed no augmentation in the proportion of the white corpuscles.

The treatment consisted in the administration of steel, a saline aperient, and the subcutaneous injection, twice a day, of a solution of the amorphous hydrochlorate of quinine (one part to five of water). After each hypodermic injection also an ice-bag was applied for several hours over the spot injected, whereby Mosler considers inflammatory action to have been prevented. After persevering in this course of treatment for sixteen days a marked reduction of the spleen was noticed, the two dimensions having fallen to eleven and eight centimètres respectively. Encouraged by this, the treatment was continued, and, in spite of many uncomfortable symptoms, was carried on until the 21st of March—that is, for the space of three months—when, finding no further progress, Mosler ceased with the quinine injections, and determined to deal locally with the diseased viscus.

With a view to this he applied an ice-bag over the spleen for several hours, with the object of exciting contraction of the spleen and the reduction of the blood in its tissue. This done, he pressed the spleen as firmly as possible forwards against the abdominal parietes, and then injected twenty-two drops of a two-per-cent. solution of carbolic acid. The greater resistance of the spleen, after the piercing of the external tissues, was felt by the instrument, and the passage of the injection was attended by violent pain. This last occurrence led the operator to inject subcutaneously in the same spot two centigrammes of morphia, and to keep the ice-bag applied for the next twenty-four hours. No abdominal distension and no elevation of temperature followed, and on the 23rd of the month the pain had ceased, and the patient sat up the whole day. She was treated with

iron and quinine, and in the following month (April) was in tolerable health. Soon after the injection Mosler satisfied himself there was some reduction in size of the spleen; but subsequently it remained stationary in its dimensions, and the quinine and iron seemed to exert no influence upon the malady. Mosler therefore resolved again to inject the spleen, but on this occasion to use a solution of arsenic, employing one part of Fowler's solution to ten of water. The operation was conducted in the same fashion as before, but the pain was less, and the ice-bag applied for only twelve hours. Indeed, no reason against a repetition of the operation appeared.

In the middle of May the patient exhibited distinct improvement; the anæmic cardiac murmur had disappeared; the spleen had retreated backwards, and now measured ten centimètres by five. In June there was a progressive improvement in health, coupled with further reduction of the diseased organ.

As Professor Mosler remarks, the foregoing case proves the feasibility of injecting the substance of the spleen. He would enjoin before operation the use of measures to reduce the blood contained in the tissues of the organ, and can advise nothing better than quinine, particularly when given hypodermically, and over a considerable interval. Several hours before the operation the ice-bag should be applied. The injection should be attempted only in those cases where the spleen lies immediately, or else can be pressed close, beneath the abdominal parietes. Hueter's experiments induced Mosler to use the dilute carbolic acid, as they went to show that this substance could be introduced in a very dilute form into the splenic parenchyma without ill-consequences. He feared to use a solution of quinine on account of the violent irritation of the tissues caused by that substance. The subcutaneous injection of arsenic had been found by himself useful in splenic tumours, and consequently he resorted to it in the above case. Czerny has also employed arsenic successfully in the way of injection into the substance of malignant lymphoma of the glands. Lastly, it may be argued that a further reduction in the dimensions of the splenic tumour of the woman operated upon may be attempted by future operation; and it is very desirable that this mode of treatment be tried in suitable cases of such very intractable disease.—*Deutsches Archiv*, March, 1875.

Pathology of Carcinoma.—Professor Beneke, led to the subject by the well-known London discussion, examines at considerable length the pathology of cancer, and ranges himself among the supporters of the doctrine of its constitutional origin. What is meant by constitutional tendency he considers to require explanation. Such a tendency may originate in an alteration of the fluids or of the solids of the body, or of both together. The humoral pathology has been most in favour, and an hereditary diseased material, or otherwise a pathological accumulation of excrementitious matter, such as uric or lactic acid, has been assumed to be productive of the constitutional defect. The former idea of hereditary morbid material

is entirely hypothetical; the latter notion, though wanting in demonstration, bears more the aspect of truth; inasmuch as observation proves that a constitutional disorder may be set up by an alteration of the nutritive fluids, as happens, for instance, when the relative proportion of the constituents of the blood has suffered change. Analogy shows in the vegetable world how great a difference in the specific characters of plants, raised from seeds apparently precisely alike, originates from presumably a minute difference in the proportion of their elementary materials. There is, in Beneke's opinion, a great neglect, on the part of observers, of the less prominent and apparently less important constituents of the animal tissues, and too little attention given to the consequences of alterations in their proportions. Although, however, varied proportion of elementary parts has an influence in determining a constitutional tendency, it is not sufficient to explain it. Associated with it is the influence of inborn or acquired departures from normal structure and function—an influence beyond dispute. As examples of structural variations peculiar to individuals are, the greater or less dimensions of the respiratory apparatus, or of the liver, in relation to the other organs, and the very considerable variations in the capacity of the blood-vessels. Such differences, whether associated with humoral changes or altered proportion of the tissue-elements, are known to predispose to peculiar diseases, and they are likewise conditions known to be hereditary.

Beneke's next inquiry is, whether either or both of the above classes of abnormal phenomena belong to the history of cancer. The first circumstance noticeable in respect to this inquiry is, the usually well-nourished condition of cancer patients, when not reduced by the consequences of operation or of exhaustion. This fact he adverts to as employed by Mr. Campbell de Morgan as an argument against a constitutional diathesis, or the presence of blood disease. But he observes that it by no means indicates the absence of blood changes of a morbid character. The blood of such persons may well be considered absolutely or relatively overcharged with formative matter; or it may be one or several constituents have unduly augmented, and thereby supply an equally efficient cause of pathological changes as a deficiency, or an abnormal mixture, would do.

A second circumstance noted is, the higher or stronger development of the osseous system in cancer subjects, accompanied with (as Beneke's own experiments show) an actually increased proportion of earthy phosphates. Whatever may be the case in the later stages of exhaustion, there is no excess in the earlier phases of cancer either of oxalates or of phosphates in the urine.

A third feature is, that the arterial system is more largely developed in cancer patients; the calibre of the arteries being greater than usual. Of this fact Beneke has satisfied himself by actual comparative measurements made in 200 bodies. In the case of tubercular and scrofulous subjects, on the contrary, a narrowing of the arteries obtains.

A fourth constitutional factor to be mentioned is the tendency to the production of fat, either at the commencement of the disease or else at an earlier period of life. In connection with this circumstance must be remembered the greater liability of women to cancer, and at the same time their greater tendency to accumulate fat. An excessive development of adipose tissue is common in mammary cancer, and, where the disease has its seat in the digestive organs, also in the omentum, the appendices epiploicæ, and the mesentery. In like manner in hepatic cancer an increased volume of the liver is observable, together with much bile, and often likewise gall-stones—conditions further indicative of a propensity to the formation of fat.

A fifth point for remark is the decidedly hereditary character of cancer as a sign of constitutional diathesis. Mr. Campbell de Morgan contends, indeed, that this hereditary character offers no proof of constitutional predisposition, because we find hereditary features in a family and other inherited peculiarities which cannot be attributed to blood-taint. But in this reasoning Mr. De Morgan is wrong; he argues against a blood-taint or a peccant matter as if it were the necessary element in the production of a constitutional diathesis, and an assumed necessary entity on the part of his opponents; whereas the latter recognise the necessity of no such special *materies morbi* to account for the constitutional diathesis, but point to altered conditions and proportions of the humours, and to inherited or acquired changes of anatomical structure and function when inherited, originating in all probability in some almost inconceivable changes in the germinal matter of the ovum.

The apparent antagonism of cancer to tuberculosis and scrophulosis is a sixth argument in favour of constitutional proclivity. A seventh may be adduced from the fact of the richness of carcinomatous deposits in the so-called myelin and cholestearin. The abundance of these materials is so much the greater in proportion to the softness and the cellular consistence of the cancer.

As a further argument is the especially significant fact, that the inoculation of cancer-cells has never succeeded to produce cancer. And, lastly, cancer patients are peculiar constitutionally by seldom possessing a nervous or sanguine temperament, being, on the contrary, remarkable for a lymphatic temperament and for defective mental and physical energy.

Grouping these arguments together in favour of the constitutional character of carcinoma, Beneke concludes that they possess so much weight that the objections of Mr. De Morgan to the hypothesis fail to overturn them. At the same time he recognises the force of the arguments against them, and disposes of these in detail more or less completely. After so doing he returns to the question of the nature of a constitutional defect. A constitutional alteration is not, he writes, as a rule, and as commonly apprehended, the result of a single, determinate deviation from the normal condition, either in the composition of the humours, or in the anatomical mechanism; and much less is it the consequence of an imaginary blood-taint. On the contrary, it is, in most instances, a consequence of the con-

currence of various departures from the healthy standard; and, according to the manifold combinations among the various deviations, will be the abnormal results in the shape of various diseased conditions. In connection with this view must be accorded a very great importance to the undoubted fact of the inconstancy and the difference in intensity of the several elements (integrals) of constitutional anomalies. It shows the possibility of numerous differences among such disorders, the impossibility of the transformation of one diseased state into another; the latent condition and the temporary exacerbations; the curability of the malady in one case and its incurability in another. The greater the abnormality of the anatomical irregularities, coupled with concurrent alterations, the less will be the chance of cure.

It is highly probable that, in respect of the whole group of constitutional diseases, *e.g.*, carcinoma, tuberculosis and gout, that certain deviations from the normal are characteristic and constant, whilst others are immaterial and inconstant, and that in this way modifications of the entire resultant lesion are brought about.—*Deutsches Archiv für klin. Medicin*, July, 1875.

REPORT ON MIDWIFERY, DISEASES OF WOMEN, AND DISEASES OF CHILDREN.

By Alfred Wiltshire, M.D., M.R.C.P. Lond.,

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Midwifery.

1. *On Chloroformization of Women during Labour, and its Influence on the Fœtus.* By Dr. Zweifel ('Berliner Klin. Wochenschrift,' No. 21, 1875).
2. *On Temperature in Puerperal Eclampsia, and the Clinical Indications it furnishes.* By Dr. Bourneville ('Archives de Tocologie,' April, 1875).
3. *Complete Inversion of the Uterus after Delivery.* By Dr. Voelckel ('Berl. Klin. Wochensch.,' March 15, 1875).
4. *Chloral as an Anodyne in Labour.* By Dr. Chiarleoni ('Gazetta Medica Italiana-Lombardia,' February, 1875).
5. *Pregnancy and Labour in Epileptic Women.* By John S. Parry, M.D. ('Amer. Journ. of Obstetrics,' August, 1875).
6. *A Clinical Contribution to the Treatment of Tubal Pregnancy.* By T. G. Gaillard Thomas, M.D. ('New York Med. Journ.,' June, 1875).
7. *Contributions to the Determination of the Diminution of the Uterus after Delivery.* By A. Serdukoff, M.D., of Moscow ('Edin. Med. Journ.,' May, 1875).

8. *Apparent Peculiarities of Parturition in the Negro Race.* By J. T. Johnson, M.D. ('Amer. Journ. of Obstetrics,' May, 1875).
9. *On the Uterine Souffle and Fœtal Heart.* By James Cumming, M.D. ('Edin. Med. Journal,' September, 1875).
10. *Clinical Memoir on Turning in Pelves narrowed in the Conjugate Diameter.* By William Goodell, M.D. ('Amer. Journ. of Obst.,' August, 1875).
11. *Puerperal Fever.* By Leopold Putzel, M.D. ('Amer. Journ. of Obst.,' August, 1875).

1. It has been generally believed that chloroform exhibited to women during labour exercises no particular influence on the fœtus —an opinion which the following recent observations of Dr. Zweifel do not confirm. Zweifel has distilled the placentæ of women who have had chloroform during labour, and has obtained the following positive result:—The placenta of a woman who had chloroform given her for twenty minutes, one hour before the birth of the child, furnished characteristic chemical indications of the presence of chloroform. On this question Dr. Zweifel has also made use of some valuable observations, as yet unpublished, of Dr. Mering, made in Hoppe-Seyler's laboratory, which shows that the urine of persons who have been chloroformed contain a substance reducible by Fehling's solution, and turning to the left under the action of polarised light, but incapable of undergoing fermentation. This substance Zweifel sought for in the urine of newly born children, with the following results: —In twenty five newly born children whose mothers had not had chloroform during labour he did not find the least trace of this substance; whilst in five children whose mothers had had chloroform during labour he did find it in the urine. It appears, therefore, that chloroform may affect the fœtus in utero.

2. Dr. Bourneville thus formulates the conclusions at which he has arrived from the careful observation of a number of cases:

"1. In the eclamptic state the temperature rises from the beginning to the end of the attack.

"2. In the intervals of accession the temperature maintains a high figure, and at the moment of convulsions the mercurial column registers a slight ascension.

"3. Lastly, if the eclamptic state ends in death, the temperature continues to rise and attains a very high figure; if on the contrary the accessions disappear, and the coma diminishes or ceases definitely, the temperature progressively lowers and returns to the normal figure."

Dr. Bourneville remarks also that, in addition to the valuable information afforded by the thermometer in respect of prognosis and treatment, it furnishes valuable diagnostic indications. He states that in *true uræmia*, whether it occur in men or in women, whether it be caused by an affection of the kidneys or by obliteration of the ureters, whether it assume the comatose or convulsive form, the temperature is always progressively lowered, sometimes falling very low.

He adds—" From the first we note a LOWERING of the temperature in URÆMIA and an ELEVATION of the temperature in PUERPERAL ECLAMPSIA. In the course of uræmia the temperature is progressively lowered, whilst in the course of the eclamptic state it rises more and more from the onset of the accessions, and that with great rapidity. These differences are accentuated at the approach, and even at the moment of death: in uræmia the temperature descends very low, even much below the normal figure; in puerperal eclampsia, on the contrary, it attains a very high figure."

3. Dr. Voelckel reports a case of complete inversion of the uterus occurring fifty-two hours after delivery. A primipara, æt. 32, was seen thirty-six hours after the commencement of labour and ten hours after the membranes had ruptured. The head presented in the first position, was impacted, and a large caput succedaneum had formed. The child was delivered by forceps. The uterus did not contract well, and the placenta was forced out by Credé's method. Severe flooding set in, which was arrested with difficulty by cold water injections and the administration of ergot. On the morning of the third day severe afterpain set in, and with an ardent desire to micturate; hot fomentations were applied. The pain became stronger and more frequent towards evening. Suddenly with a strong expulsive effort a globular body was forced out of the vulva, followed by a gush of blood, and in a moment the patient was dead. Dr. Voelckel saw the patient four hours after; the uterus was found completely inverted, lying outside the vulva. No necropsy was allowed.

4. Dr. Chiarleoni confirms the statements made by Dr. Playfair and others in this country and abroad that chloral lessens the pains of childbirth. Dr. Chiarleoni believes the drug to promote uterine action. He thinks it especially admissible in uræmic convulsions.

5. Dr. Parry's paper is illustrated by cases, and concludes as follows:

"In regard to the relations existing between pregnancy, parturition, and epilepsy, experience and the study of the literature of the subject appear to warrant us in making the following statements:

(1.) Epileptics rarely have convulsions during labour. They are not more liable to puerperal convulsions than healthy women. Labour in them is, as a rule, not more unfavorable than in healthy women.

(2.) In the exceptional cases, in which violent epileptic convulsions occur during labour, it is not decided whether it is best to hasten delivery or to trust to nature.

(3.) Pregnancy may be the immediate cause of epilepsy. In these cases fits rarely occur during labour, and the disease is immediately arrested by parturition, but it will almost always reappear whenever the woman becomes pregnant.

(4.) Either form of epilepsy may result in the death of the fœtus, but convulsions of this kind are not as likely to destroy the child as are those which may be correctly designated puerperal.'

6. Dr. Thomas prefaces the subject matter of his very interesting

paper by some suggestive and valuable remarks. With the courage of a conscientious and experienced physician he honestly says, with reference to the exact position of extra-uterine pregnancies, "After careful examination of every case of extra-uterine fœtation to which I have had access I am unable to substantiate the position, and yet I am inclined to believe that, in the commencement of its development, the impregnated ovum never attaches itself to or draws its nourishment from any other parts than those lined by the mucous membrane of the uterus or tubes."

The case, briefly epitomised, was as follows: A lady, æt. 32, married six years, had never had any family when symptoms of pregnancy supervened. All went well for about two and a half months, then after a few days uneasiness and agonising pain suddenly came on during the night. The patient remained ill with more or less pain for a fortnight, when Dr. Thomas was called to see her in consultation. Physical examination showed that the uterus was somewhat enlarged, measuring three and a half inches, and slightly anteverted obliquely to the right.

The vagina was soft, elastic, and enlarged, as it is during pregnancy. To the left of the uterus was a tense elastic cyst, filling the whole iliac fossa, and extending downwards to the level of the internal os uteri. This cyst was slightly movable and sensitive. Ballottement carefully practised revealed feeble but distinct "evidence of a very light body which was thrown upwards and fell upon the floor of the sac." Dr. Thomas from these facts diagnosed left tubal pregnancy at the end of the third month of development. Believing rupture to be imminent from the nature of the symptoms, Dr. Thomas urged immediate surgical interference. This he carried out in the following manner, selecting elytroraphy as the means by which he would open the sac. To prevent undue hæmorrhage from the incision into such vascular parts as he was about to deal with, Dr. Thomas used a platinum knife with the galvano-caustic battery. He first put the upper part of the vagina over the cyst on the stretch, by means of tenaculum fixed in the cervix uteri and vagina, then, with the knife at a white heat, he opened the cyst, giving exit to some straw-coloured pinkish fluid. Introducing the index finger a fœtus was found and extracted by podalic version as far as the head, which became arrested, and was removed only after the application of a pair of long-handled placental forceps. Dr. Thomas then attempted to remove the placenta by gentle traction and detachment. At this point the first difficulty which had attended the operation showed itself. Not knowing the exact manner in which the placenta is attached in tubal pregnancy, Dr. Thomas proceeded very cautiously, and when about half the placenta was separated very severe hæmorrhage took place, so much so that, tearing off the detached portion, the operator at once passed a gum-elastic catheter, and injected a solution of persulphate of iron into the sac. This instantly checked the flow of blood. Being unable to make use of a drainage tube, Dr. Thomas inserted a long tent of carbolized cotton, saturated with a solution of persulphate of iron. Morphia was injected

subcutaneously. All went well until the fourth day, when the tent was removed, and symptoms of septicæmia soon showed themelves. These yielded to constant injections of carbolized water. On the fifteenth day the rest of the placenta came away spontaneously. In spite of trivial drawbacks the patient did very well, and at the end of six weeks Dr. Thomas could scarcely find the exact position of the cicatrix.

Dr. Thomas justly lays much stress upon the value of ballottement, but philosophically adds that "on no single sign, however, should undue reliance be placed." He makes no mention of purpling of the vagina, a symptom which in our own experience we have found of value. Dr. Thomas states that during the first sixteen years of his practice he saw no case of extra-uterine pregnancy; then he saw four in one month. During the past seven years he has met with nine.

Dr. Thomas's case, operation, and remarks are alike interesting and instructive. The all-important point in respect of the operation, whether the sac be opened through the vagina or by gastrotomy, is, we apprehend, the treatment of the placenta. Shall this be removed or no? Though Dr. Thomas's case is encouraging in the affirmative, there are doubtless very great risks attending its removal on account of hæmorrhage, while on the other hand the risks of septicæmia, if it be left to disintegrate, are by no means slight. On the whole we should, in the light of present experience, be disposed to follow the latter practice; always taking the utmost precaution to ensure a sufficient opening for the irrigation of the sac by means of carbolized, iodized, or other antiseptic solutions. But before we can be entitled to express any very decided views upon this point, we must wait for the light of further knowledge and experience. Dr. Thomas has added to both, and, with Koeberle and others, has done much to advance the science of medicine in respect of these serious, though happily somewhat rare cases.

7. Dr. Serdukoff contributes an elaborate article on this subject, based on measurements carefully made by himself on 150 women.

The mode of measurement will doubtless be objected to by some, and it is certainly open to criticism, as being inherently somewhat deficient in exactness. Dr. Serdukoff relied on external measurements, preferring these, for various reasons, to internal ones. He concludes from his investigations that involution of the uterus is not complete until the lapse of from four to six weeks.

The author's conclusions are as follows:—

(1.) Involution of the uterus goes on more rapidly during the first few days of the puerperal period than it subsequently does.

(2.) Involution of the uterus of healthy women goes on well and with regularity.

(3.) Involution, where the uterus is the subject of diseases, such as metritis, endo-metritis, or parametritis, goes on more slowly, and this varies with the amount of disease.

(4.) The permanent contraction which takes place during the first

few hours after delivery is a common occurrence. When it passes off an increase in size begins to take place.

(5.) In women delivered at the full time involution goes on more quickly and regularly than in those prematurely confined.

(6.) Length of labour retards involution.

(7.) In adult primiparæ involution of the uterus goes on very regularly, but more slowly than in young primiparæ. In aged multiparæ involution does not go on so well.

(8.) In women who suckle their children involution during the first four days does not go on so quickly as in those who do not nurse. But, subsequently, the involution is quicker, though less regular.

(9.) Afterpains are not necessary for a favorable involution; in fact, we are as well without them.

(10.) In order to determine the involution of the uterus, its breadth only should be measured.

(11.) Involution of the uterus goes on proportionally in length as well as breadth.

(12.) Super-involution and sub-involution occur as distinct uncomplicated pathological conditions.

8. Dr. Johnson having at Washington large opportunities for observing parturition in negresses, has here given a comparison between this function as observed in negresses and in white women, based on two thousand cases of labour occurring in negro women. He uses the statistics of Churchill as the standard of comparison among white women.

Length of Labour.—This appears to be on an average shorter in negresses than in white women.

Presentations.—The head appears, from the statements given, to present much more frequently in negresses than in white women; abnormal presentations are, therefore, less frequent among the blacks.

Triplets occurred once.

The period of convalesence is proverbially short in coloured women.

Placenta prævia occurred but once in the 2000 cases.

Flooding during labour occurred but twice.

Flooding after labour occurred four times. None of the cases were fatal.

Puerperal fever occurred four times.

Operations.—(*a*) The *forceps* were required but four times. *A propos* of this Dr. Johnson, in some excellent remarks, says: "The low development of the anterior cerebral lobes, and consequent shortening of the fronto-mental and occipito-frontal diameters of the fœtal head diminishes the suffering of the patient. The diameters being shorter the resistance is less, and the greater occipital development is accommodated by the peculiar shape of the negro pelvis."

(*b*) *Version.*—This operation was performed three times. "Once in the Freedmen's Hospital, when the left hand, right foot, and funis presented, with the head crowded down upon them; once when rupture of the uterus occurred, and the child had to be drawn

back into the uterus from the abdominal cavity previous to version, and once in a consultation case, where forceps and craniotomy both failed, and the child had to be turned finally and brought down by the feet." In reference to the last case it is interesting to note that Dr. Johnson says that this is the only instance in which he has met with deformity of the pelvis in a negress. Here there was contraction of the pelvis antero-posteriorly.

An able and exhaustive discussion upon the important question of racial peculiarities of the pelvis is here given by Dr. Johnson, who concludes that there are beyond doubt "race peculiarities belonging to the negro female pelvis," the chief point established being "that this antero-posterior diameter of the inlet of the female pelvis in the negro is longer than in the European female." This he believes corresponds to the shape of the average negro head.

Puerperal Accidents.—Under this heading Dr. Johnson states that stillbirths and abortions are comparatively rare. There were only seven stillbirths in his 2000 cases.

Umbilical Cord around the neck of the infant is found to be very frequent in negresses, occurring once in every six cases.

Rupture of the Uterus occurred but once. Churchill's statistics give once in every 1318 cases. In Dr. Johnson's case the woman recovered and subsequently became pregnant. Dr. Johnson does not state the cause or character of the rupture.

Puerperal Convulsions occurred eight times in Dr. Johnson's 2000 cases. Leishman gives the frequency as once in every 350 cases in white women. Dr. Johnson states, however, that his figures are regarded as exceptionally high by other physicians who have experience of parturition in negresses.

9. Dr. Cumming has commenced in the September number of the 'Edinburgh Medical Journal,' an essay on the above subjects. Thus far he has only dealt with the uterine souffle; the mode of production of which is discussed chiefly from the historical standpoint. The conclusions of Dubois, Depaul, Cazeaux, Kilian, Matthews Duncan, Tanner, Tyler Smith, Leishman, and Schroeder, are supported. The view maintained by the above-named authorities is doubtless the correct one, viz., that the souffle is properly of uterine and not of placental origin, and that it is produced in the vessels which ramify in the uterine walls.

10. Professor Goodell contributes an excellent practical paper on this subject, based on his own experience. He relates several cases in which he resorted to version with the best results. His thoughts, put into a practical shape, are offered in the followlng general propositions:

"(1.) Turning should generally be preferred to the lashing of the forceps handles.

"(2.) In pelves uniformly contracted the forceps is the better means of delivery.

"(3.) In pelves narrowed in the conjugate diameter turning should be resorted to whenever a half-hour's faithful trial with the forceps fails to make the head engage.

"(4.) In pelves whose conjugates range from 2·75 to 3·25 inches turning should be the initial step."

11. This much discussed subject is dealt with by Dr. Putzel in a concise article in which he upholds the specific view or essential nature of the disease. Dr. Putzel speaks certainly with more authority than most physicians can pretend to, since his observations are based on twenty-two (22) post-mortems, witnessed by himself, of patients several of whom he had the opportunity of observing during life. Dr. Putzel maintains the view lately so ably advocated by our distinguished *confrère* Professor Fordyce Barker during the discussion of the subject at the Obstetrical Society, viz., that the disease is of a "specific" or "zymotic" nature.

Diseases of Women.

1. *On the Use of Chloral Hydrate in Carcinoma Uteri.* By Dr. Fleischer. ('Med.-Chir. Centralblatt,' ix, 1875).
2. *On the Combination of Chlorosis with Aplasia of the Female Sexual Organs.* By Dr. Ernst Fränkel ('Arch. für Gynæk.,' Band vii, Heft 3).
3. *On Obliteration of the Vagina for the Cure of Incontinence of Urine due to great Destruction of the Vesical Walls.* By Dr. Herrgott, of Nancy ('Arch. de Tocologie,' June, 1875).
4. *The Mucous Membrane of the Uterus, with especial reference to the Development and Structure of the Decidua.* By G. J. Engelmann, A.M., M.D. ('Amer. Journ. of Obstetrics,' May, 1875).
5. *Three Cases of Successful Removal of Fibro-Cystic Tumour of the Uterus.* By Thomas Keith, M.D. ('Lancet,' May 15, 1875).
6. *On the Arrest of Metrorrhagia by the Application of Heat to the Lumbar Region.* By Dr. Noël Gueneau de Mussy ('Annales de Gynæcologie,' July, 1875).
7. *On Ovariotomy.* By James R. Chadwick, M.D., Boston, U.S.A. (Pamphlet).
8. *Successful Ovariotomy in a Case of Cyst of the Ovary, lasting Thirty-four Years, which had been punctured twenty-four times.* By Dr. Bouyer ('Gazette Obstetricale,' June, 1875).

1. Dr. Fleischer recommends the use of chloral hydrate locally in carcinoma uteri. After thoroughly washing out the vagina, he passes up to the cancerous surface a piece of cotton wool dipped in a solution of chloral hydrate (two drachms to three ounces); this is repeated every two hours. After two or three applications the character of the pain is altered, and the discharge becomes less offensive. He prefers administering chloral as an anodyne by the rectum rather than by the mouth; its effect can be better regulated, and it does not confine the bowels as morphia does.

2. Dr. Ernst Fränkel concludes a paper on the subject with the following summary: That chlorosis and imperfect development of

the genital organs are frequently, though not exclusively, combined with a dwarf-like condition of the heart and the aorta. The influence which the sexual apparatus exercises upon the body may also determine the character of the blood, that is to say, that sexual aplasia may be the prime originator of chlorosis. That form of chlorosis which is not associated with imperfect development of the vascular system is permanently curable. The so-called menorrhagic form of chlorosis is met with as well in deficient as with excessive development of the sexual organs.

[This appears to accord with Virchow's well-known writings on the subject.—A. W.]

3. Professor Herrgott discusses this operation pretty fully in the article indicated. He claims for France the honour of originating it; the operation being first practised by Vidal de Cassis in 1832. Since then, however, the advances in this department of plastic surgery have been very great; valuable contributions being made by surgeons in all parts of the world, and especially by our American brethren. Obliteration of the vagina is no doubt a most valuable operative procedure, and gives immense relief and comfort to those who are afflicted with the distressing lesion which calls for it.

4. In 1872 a paper on this subject, based on researches made by Dr. Engelmann in conjunction with Dr. Kundrat, was read by the latter gentleman before the "Gesellschaft der Aerzte" of Vienna, and published in Stricker's 'Medizinische Jahrbucher' for 1873. Some of the views stated in this paper were developed after Dr. Engelmann left Vienna, and as he finds he cannot endorse them he has published his views in the able paper now before us. The wealth of material available to Dr. Engelmann both in Vienna and in Berlin was very considerable and has formed the basis of this elaborate communication, which is largely illustrated by microscopic drawings copied from the original paper by Kundrat and Engelmann in Stricker's 'Jahrbuch.'

Our space will not permit us to do more than very briefly refer to Dr. Engelmann's researches; we must, therefore, in justice to the author, ask those who are interested in the subject to follow the whole of the arguments adduced in the original paper.

We may say, however, that Dr. Engelmann dissents from the views ordinarily accepted respecting the character and behaviour of the uterine mucous membrane, whether in the impregnated or unimpregnated state. He does not agree with the view that the mucous membrane of the uterus is shed at every menstrual period; and he asserts that the evidence we have of the simultaneous occurrence of menstruation and ovulation is, in his judgment, conclusive. With respect to the very important subject of the formation of the placenta Dr. Engelmann avers that the uterine, or maternal, part of this organ is formed "only of the most superficial of the dense upper layers of the serotina and in its greatest extent of the protuberances which spring from these and envelop the ramifications of the villi in their growth." He denies that the chorion villi dip into the glandular ducts.

Dr. Engelmann's able paper is deserving of careful study.

5. Dr. Keith concludes the narration of these cases with some valuable remarks. He states that these are the only instances in which he has interfered with uterine tumours by abdominal section. He generally declines to operate. These cases are, he says, enough to satisfy him that the removal of an enlarged uterus and ovaries is not an operation to be lightly undertaken. Hæmorrhage may be severe, and the personal attendance necessary in each of the three cases was, Dr. Keith remarks, greater than half a dozen ordinary cases of ovariotomy. Still Dr. Keith is hopeful that, with the progress of science, something may be practicable for the relief of patients afflicted with these growths, and he concludes as follows:

"Though the greater number of uterine tumours, if not malignant, give little trouble, rarely interfering with life, and often not even with the comfort of the patient, yet in a certain number they cause the subjects of them to lead useless, dependent, miserable lives, full of suffering, ending only with life. I hope the time is not far distant when many of these unfortunates will look to surgery for relief with as much confidence as those afflicted with ovarian disease now do."

6. Dr. Gueneau de Mussy having recently made use of this method of treatment, records his experience of it. He gives particulars of two cases, in both of which cessation of hæmorrhage followed the application of heat to the lumbar region by means of Chapman's bags; but also, it is very important to note, some singular and rather alarming phenomena ensued.

In the first case the only pathological conditions noted about the uterus were the existence of pelvic adhesions on the right side and some ovarian tenderness. Various means were unsuccessfully employed to arrest the hæmorrhage. Dr. de Mussy then resorted to the bags of hot water, which in one day materially diminished the flow, and in two days arrested it. By this time the patient complained of pains in the lower portion of the body and in the head, and very shortly she was seized with intense dyspnœa and spat an ounce or two of blood. Examination of the chest revealed subclavicular dulness on the right side, and sub-bronchitic expiration. In nine or ten days this "adenopathie bronchique," as Dr. de Mussy terms it, had materially improved. In another month the menses reappeared, the flow being considerable, but in view of past experience with the hot bags Dr. de Mussy waited five days before interfering. Then, however, as the patient had faintings and the loss of blood amounted to a flooding, the hot bags were again applied, and again the application of them was followed by the same phenomena; the metrorrhagia was arrested, iliac and sub-pubic pains increased, and hæmoptysis recurred. Some time elapsed before the pulmonary symptoms had disappeared, and in fact they did not entirely do so until the warm weather came. The menorrhagia was less at the subsequent periods.

In the second case there had probably been a miscarriage two months before treatment was commenced. The uterus was rather

large and was retroverted. Here the employment of the hot bags was attended by arrest of the hæmorrhage, but also by flushings, a sense of weight about the head, and on several occasions she was on the point of losing consciousness. The hæmorrhage was cured, however, and the physical condition of the uterus was found on examination some days afterwards to have materially improved.

Dr. de Mussy says that he was led to the employment of Chapman's bags not only by the failure of all treatment, local and general, already adopted, but also because of the confidence that Trousseau used to have in the use of hot injections for metrorrhagia.

In reference to the causation of the uterine hæmorrhage, and the sudden supervention of hæmoptysis, he says that he is inclined to regard the uterine hæmorrhage as being caused by the presence of tubercle in the broad ligaments or ovaries, thus expressing the hæmorrhagic tendency of tubercle which is encountered whatever may be its seat. The pulmonary lesion he regards as existing, but being masked by the uterine hæmorrhage, becoming active, however, after the suppression of that powerful derivative.

7. Dr. Chadwick, in a paper on two interesting cases of ovariotomy, read before an American medical society a few months ago, makes mention of a new procedure adopted by him to facilitate diagnosis in an obscure case of abdominal tumour which was complicated by by ascites. He put the patient in the knee-elbow posture, with the idea that if the tumour were a fibroid its weight would cause it to sink in the ascitic fluid until it rested on the abdominal walls. A thin-walled ovarian cyst, on the other hand, would be scarcely heavier than the ascitic fluid; it would consequently not change its position materially on change of the patient's posture. Two contingencies might nullify the inference to be drawn from these indications—the presence of pelvic adhesions, and a partially solid character of an ovarian tumour. The method is of course only applicable to small and medium-sized tumours surrounded by a considerable amount of ascitic fluid. Governed by these considerations Dr. Chadwick diagnosed in the second of his cases an ovarian cyst, probably unilocular, free from adhesions, and with no solid part, but surrounded by a varying amount of ascitic fluid. The accuracy of this diagnosis was verified at the operation.

In the same paper Dr. Chadwick suggests that when puncture of the intestines by means of the aspirator becomes necessary for the relief of flatulent distension of the abdomen, advantage might be taken of the presence of the canula in the bowel to administer brandy, beef-tea, &c., after the escape of the gas.

Dr. Chadwick's excellent paper is characterised by a pleasing calmness of tone. It is, moreover, a very useful contribution to the subject of ovariotomy.

(8.) Dr. Bouyer, of Angoulême, relates the particulars of an interesting case which occurred in the person of a woman aged 49 years, who had shown signs of unusual abdominal enlargement at the age of 14, but who was not subjected to puncture until her 25th year.

When she was seen by Dr. Bouyer in August, 1874, he diagnosed an ovarian cyst without adhesions, and advised ovariotomy. This he successfully performed. In spite of the frequent punctures no adhesions existed. The pedicle was transfixed in the abdominal wound by means of a pin passing through it and the abdominal parietes. The patient got almost well, but, in spite of warnings, persisted in going to stool in severe weather very lightly clad, and thus got chilled, and died of pneumonia twenty-one days after the operation.

Diseases of Children.

1. *On Aphasia in Children.* By Dr. A. Clarus ('Jahrbuch für Kinderheilk.,' pp. 369-400, July, 1874).
2. *Cerebroscopic Review of the Paris Hospital for Sick Children in* 1874. By M. Bouchut ('Gazette des Hôpitaux,' January, 1875).
3. *Skin Eruption in a Child caused by Bromide of Potassium taken by the Mother.* By Dr. Tilbury Fox ('Lancet,' Nov. 7, 1874).
4. *Imperfect Teeth and Zonular Cataract.* By Mr. Jonathan Hutchinson ('Lancet,' March 6th, 1875).
5. *On the Treatment of Prolapsus Ani in Children.* By Dr. Gualtiero Lorigiola and Baron von Langenbeck.
6. *On Croup and Diphtheria.* By Sir William Jenner, Bart., M.D., F.R.S., and Dr. George Johnson, F.R.S. ('Lancet,' January 2nd and 16th, 1875).
7. *Purpura Hæmorrhagica, transmitted from the Mother to the Fœtus.* By Professor Dohrn ('Arch. für Gynæk.,' Band vi, p. 486, 1874).
8. *Laceration of the Navel String.* By Dr. W. Pfannkuch ('Arch. für Gynæk.,' Band vii Heft, 1, 1875).
9. *Acute Pemphigus of the Newly-born and of Young Children.* By Ladislas Faloy, M.D. ('Annales de Gynæcologie,' July, 1875).
10. *Successful Ovariotomy in a Child seven and a half years old.* By George Cupples, M.D. ('Richmond and Louisville Med. Journ.,' Jan., 1875).
11. *On the Breasts of Newly-born Infants.* By Dr. de Sinety ('Arch. de Tocologie,' June, 1875).
12. *On Trismus Nascentium.* By Dr. Wilhite ('Amer. Journ. of Med. Sc.,' April, 1875).

1. The interesting lesion which is the subject of this memoir has been largely studied in adults by many very able physicians both in this country and abroad, but so far as we know special observations of the affection, as it is seen in children, have not hitherto been made, or at least have not yet been published in a connected form.

The following is a condensed summary of the principal groups of diseases in which aphasia has been observed in children:

i. *Aphasia in idiot and deaf-mute children.*—May be congenital or acquired; is caused by cerebral lesions; in congenital aphasia

the most frequent alteration is a partial atrophy of the anterior lobes of the brains. In acquired idiocy and aphasia, chronic hydrocephalus or encephalitis are met with. Aphasia may occur in early childhood from repeated attacks of epilepsy, or as a remote result of impaired nutrition. Idiotic aphasia may be explained by the absence of ideas; that of deaf mutes by default of perception of articulate sounds, and the latter may accidentally arise in cases in which deaf-mutism follows acute diseases, as measles and scarlet fever.

ii. *Aphasia consecutive to acute diseases.*—This variety is not rare; though not mentioned in treatises on children's diseases; it is especially met with after acute specific diseases, particularly after typhoid and other eruptive fevers.

(1.) Aphasia following typhoid fever. Dr. Clarus's account of this is exceedingly interesting. It appears that the lesion occurs nearly always in boys, and between the ages of eight and eleven. The epoch at which it appears is somewhat variable, but generally it occurs late in the course of the fever. In most of the cases the attack of typhoid was severe. In a fatal case which occurred in the practice of Eisenschitz no grave cerebral lesion was found at the post-mortem examination; and it is probable that the aphasia was due in this, as in the great majority of the cases, to starving of the cerebral substance from insufficient nutrition. Dr. Clarus, however, himself mentions a fatal case of his own in which ramollissement was found consecutive to embolism of the left sylvian artery.

(2.) Aphasia consecutive to other eruptive fevers. This variety is rarer than the foregoing. Here again the lesion occurs more frequently in boys than in girls, and the age varies from eight to nine years.

iii. *Aphasia consecutive to acute cerebral affections.*—(1.) Cerebral embolism. Endocarditis seems to be the most common pathogenic condition. The aphasia may be complicated by hemiplegia or hemichorea, and there may be cerebral ramollissement. It is worthy of note that, in two cases out of five mentioned by the author (those of Kelly and Lacambre), the cerebral lesion was found at the autopsy to be seated in the right hemisphere.

(2.) Aphasia after traumatic lesions of the encephalon: "cerebral commotion" is said to have caused aphasia in two cases quoted, though this condition is believed to be rare in children. These two cases recovered—one completely, the other nearly so. Fractures were associated with the other cases mentioned.

(3.) Aphasia after inflammation of the brain and its meninges. (*a*) Tubercle and abscess of the brain. The only case of this kind known to the author is one of Dr. West's. (*b*) Tubercular meningitis—one case by Bouchut. (*c*) Encephalitis and abscess of the brain—a case of Rilliet and Barthez.

iv. *Aphasia consecutive to chronic affections of the brain.*—This variety is relatively frequent. Cerebral sclerosis and atrophy may be attended by aphasia as seen in idiocy.

Cerebral tumours.—These do not appear to cause aphasia very

often, compared with their relative frequency. It appears that entozoa of the brain cause aphasia more frequently than other tumours; and Dr. Clarus gives six cases caused by hydatids within the encephalon.

v. *Aphasia in Neuroses.*—Aphasia has been known to follow convulsions in children. Dr. Hughlings Jackson has recorded two cases, in which, however, he admits the possible existence of a pathological lesion in the corpus striatum. Dr. Ogle has in this Review shown the occurrence of aphasia with chorea, and so has Bouchut in 'Bull. Gén. de Thérap.' The prognosis in these cases is favorable. Aphasia may be produced reflexly, and in particular by worms in the intestines.

Prognosis.—It is obvious that the prognosis of aphasia in children is variable, and must be subordinate to the causation of the malady. Generally speaking, when it arises in the course of acute specific diseases, the prognosis is less serious. Much the same may be said of neurotic aphasia. But in the congenital cases, and in those due to embolism or organic lesion of the brain, the prognosis is extremely grave.

As regards treatment there is none very special, except in the cases of reflex origin (intestinal worms), and perhaps in some of the traumatic cases in which spiculæ of bone are injuring the brain.

2. In a few introductory remarks Dr. Bouchut says, that the twelve years during which he has published essays on cerebroscopy have confirmed his view of the importance of lesions of the optic nerve, of the retina, and of the choroid produced by diseases of the brain, meninges, and the spinal chord. Some of the lesions discovered by the ophthalmoscope are of themselves diagnostic, as retinal thromboses, miliary aneurisms of the retinal artery, certain "steatoses" of the retina, tubercles of the choroid, &c. Dr. Bouchut says that all the neurites, neuro-retinites or choroidites are formed according to laws which he formulated long ago, and which are immutable. They are—

(1.) Neuro-retinitis of *mechanical origin*, when an obstacle to the meningeal circulation (meningitis or acute hydrocephalus, meningeal effusion, tumour compressing the encephalon, thromboses of the meningeal veins, &c.), hinders the return of blood within the cranium. Thus the serosity of the subarachnoid space descends as far as the sheath of the optic nerve, as has been established by Key and Schwalbe, compresses the nerve in such a way as to narrow the retinal artery and to retain within the eye the blood of the retinal veins.

Hyperæmia with papillary œdema is thus produced; a dilatation and a varicosity with thrombosis of the retinal veins involve more or less quickly alterations in the nutrition of the optic nerve and retina.

(2.) A *descending neuritis* when chronic inflammation of the brain or cerebellum descends by the optic nerve to the papillæ. This is the case with tumours of the brain or cerebellum.

(3.) An *ascending reflex neuritis* resulting from diseases of the

spinal cord in ataxy, chorea, &c. This is the effect of the action of the great sympathetic which takes its origin in dorsal region of the cord.

(4.) A diathetic neuritis or choroiditis which reveals tuberculosis, leucæmia, syphilis, glycosuria, &c. Hence arise those cases of tubercular neuritis and choroiditis, of syphilitic choroiditis, of leucæmic, glycosuric, or albuminuric retinitis which everybody is beginning to know of.

These are the different kinds of lesions of which Dr. Bouchut affirms anew the existence in the cases reported in his service, many of which were verified by autopsy. The long series of cases briefly narrated include cases of traumatic meningeal hæmorrhage, typhoid meningitis, tubercular meningitis, granular meningitis, meningitis with consecutive hemiplegia, tubercular meningitis and vertebral caries, cerebral hæmorrhage, tumour of the brain, acute traumatic myelitis, paraplegia with contraction, convulsions after measles, infantile paralysis, diphtheritic paralysis, chorea with optic neuritis, hemichorea, tubercular pneumonia—a most instructive and valuable series of cases all observed in a children's hospital.

Dr. Bouchut truly says that the ophthalmoscope has become as indispensable to the physician as to the oculist. He says that the evidence ascertained by the ophthalmoscope shows that chorea, considered by many physicians as a simple neurosis, is to be referred to a congestive affection of the anterior columns of the spinal marrow, and refers to his observations here given in proof of this statement.

Hysterical paralysis or paraplegia do not cause any neuro-retinal alteration, while those which are symptomatic of myelitis and spinal ataxies do. Dr. Bouchut thinks the ophthalmoscope remarkably valuable in those cases of acute general tuberculosis which are accompanied by typhoid symptoms, and which may therefore be mistaken for typhoid fever, and he gives a fatal case in which the correctness of the diagnosis made by the ophthalmoscope was fully verified by the discovery after death of tubercle throughout the body.

In illustration of the great value of ophthalmoscopic examination he says thus—

"From hyperæmia and hyperæmic swelling of the optic nerve result the diagnosis of mechanical or inflammatory hyperæmia of the brain in meningitis, in cerebral hæmorrhage, in cerebral effusions, and in some cases the diagnosis of ataxic or other spinal diseases.

"By papillary œdema, combined with hyperæmia, I recognise œdema of the meninges or impediment to the cerebral circulation, determined by meningitis, by certain cerebral tumours, by ventricular hydrocephalus, by cerebral hæmorrhage and meningitic effusions, by thrombosis of the sinues, &c.

"By neuro-retinal and choroidal anæmia I recognise the cerebral hæmorrhage of ramollissement, and if the anæmia is absolute it means death. Arteries and veins of the eye empty of blood, and the choroid mesh exsanguine, that is arrest of the cerebral and cardiac circulation.

"By optic neuro-retinitis, exudative and fatty, I recognise chronic meningo-encephalitis; the encephalitis of cerebral tumours, and the alteration of nervous substance which accompanies these tumours.

"By retinal varices and thromboses I distinguish thromboses of the meninges or of the sinuses.

"By aneurisms of the retinal arteries we recognise miliary aneurisms of the brain.

"By simple retinal hæmorrhages one recognises compression of the brain by effusion, hæmorrhagic, or other; but if these hæmorrhages are accompanied by retinal steatosis it is because there is cerebral steatosis, and this is the case in chronic albuminuria, leucocythæmia, and glycosuria.

"By atrophy of the optic nerve one distinguishes tumours of the brain and cerebral or spinal sclerosis.

"Lastly, we do not get tubercular granulations in the choroid without there being similar ones in the lungs and other organs."

We are glad to note that Dr. Bouchut promises to publish an 'Atlas of Medical Ophthalmoscopy,' which he is preparing.

3. Dr. Fox here records a fact which has not hitherto, it is believed, been observed.

A child three months old had an eruption which, at first sight, seemed to be probably due to vaccination. The child had been vaccinated about a month before it was seen by Dr. Fox, but some pimples had been observed on the cheeks about six days after the vaccination. These gradually spread, so that when the child was brought to the hospital there were acneiform spots on various parts of the body.

There was no constitutional syphilis.

Recognising the resemblance of the eruption to that caused by taking bromide of potassium, Dr. Fox made inquiry whether that drug had been taken, and it turned out that the mother had been taking it for the previous eighteen or twenty months for epilepsy.

The medicine was discontinued, and the eruption at once began to disappear. She then resumed it, and very speedily a fresh outbreak of the eruption occurred. It is curious that the mother herself had not had any eruption on the skin.

4. In an able communication read before the Pathological Society on March 2nd, 1875, Mr. Hutchinson has made some very interesting statements which deserve the attention of all who see much of children's diseases.

Mr. Hutchinson says that for some years past it has been a matter of general knowledge amongst ophthalmic surgeons that when children are the subjects of cataract they usually show also badly developed teeth; the malformation of the teeth being variously ascribed to congenital syphilis, rickets, and general defective development. Imperfect teeth of the kind referred to by Mr. Hutchinson are, he believes, met with in connection with but one form of the cataract of childhood, namely, the "lamellar" or "zonular" form. There may be fair vision with this cataract, which is believed to be invariably symmetrical. It does not appear to be associated with any particular

diathesis, and it is very exceptional to meet with it in more than one child in a family. These facts lead to the inference that it is probably the record of some temporary disturbance in the nutrition of the lens rather than the result of any permanent impairment of the patient's health. It seems to be a disease of the earlier years of childhood. The *congenital* cataract belongs, Mr. Hutchinson thinks, to an entirely different category.

The condition of teeth observed in this form of cataract is said to be wholly different from that met with in congenital syphilis, and consists not so much in alteration in the form of the teeth as in defective development of the enamel. This may occur in association with the malformations which characterise hereditary syphilis, and hence, doubtless, the confusion.

The incisors, canines, and first molars suffer most, the bicuspids escaping entirely. It appears that the *permanent* teeth alone exhibit changes which can be relied upon for diagnosis. They are pitted, dirty, broken, and display sharp edges. Non-development of enamel and erosion of the exposed dentine appear to be the especial features. A point of singular interest is, that there is usually a history of convulsions in early infancy in cases of lamellar cataract, an observation which we owe to Arlt, and which is confirmed by Mr. Hutchinson. Another important point resulting from this is, that it appears from inquiry that these defects of the teeth usually result from attacks of inflammation of the gums in early infancy, and that mercury holds the chief place in the causation of stomatitis. Should this prove to be so, it has obviously a very important bearing on the propriety of using mercury in the diseases of infancy. It appears that inquiries into cases of lamellar cataract lead to the belief that the connection between this form of cataract and fits is almost universal. Mr. Hutchinson considers the connection of the malformation with rickets entirely wanting in proof.

Mr. Hutchinson sums up his results as follows:

"1. That it is exceptional to meet with lamellar cataract excepting in association with an imperfect development of the enamel of the teeth, but that definite exceptions, in which the teeth are quite perfect, do occur.

"2. That the kind of defect observed consists in the absence of the enamel, and is shown on the incisors, canines, and first molars of the *permanent* set to the almost invariable exemption of the premolars. That for purposes of diagnosis the first molars are by far the most important, and may rank as the test teeth since they not unfrequently show the defect when others escape.

"3. That it is highly probable that the defects in the development of the teeth are usually due to the influence of mercury exhibited during infancy, although it is quite possible that other influences, attended perhaps by inflammation of the gums, may occasionally produce similar results.

"4. That teeth of the kind alluded to are met with very often in persons who are not the subjects of zonular cataract.

"5. That it is very rare to find lamellar cataracts without history of convulsions in infancy.

"6. That there is probably no direct connection between the occurrence of convulsions in infancy and the development of lamellar cataract.

"7. That whilst there is every reason to believe that the defective teeth which are met with in connection with zonular cataract are the results of mercury, the evidence seems opposed to the belief that the lenticular opacity is due to the influence of the drug.

"8. That the very frequent coincident occurrence of lamellæ with defective teeth is to be explained by reference to the frequency with which mercury is given for the treatment of convulsions in infancy.

"9. That there is no reason whatever for supposing that lamellar cataracts have any connection with hereditary syphilis.

"10. That whilst it is certainly true that lamellar cataracts are commonly met with in young persons who show general defects of development—short stature, ill-shaped heads, defective intellect, dwarfed lower jaws, or other physiognomical peculiarities—yet there is seldom any proof of the existence of rickets, whilst it is quite possible that the particulars mentioned may be due to the disturbance of the nervous system in infancy in connection with the convulsions.

"11. It is very important to distinguish between mercurial teeth and syphilitic teeth; that the peculiarities presented by each usually render this easy, whilst, however, the two are, as might have been expected, not uncommonly met with together."

5. In the "Report on Midwifery and the Diseases of Women and Children" published in the October (1874) number of this 'Review,' there was a reference to the use of the actual cautery in prolapse of the rectum in young children proposed by M. Panas, respecting which Dr. Lorigiola has kindly addressed a letter to the reporter expressing his astonishment at the use of the actual cautery in this affection, and recommending the subcutaneous injection of a solution of strychnine into the tissues near the anus.

Dr. Lorigiola says he has for a long time used with great success a solution of sulphate of strychnia, and that he has thus treated six cases, none of which required more than two injections. Dr. Lorigiola's formula for the solution is as follows:—

℞ Sulphatis Strychniæ.
Centigramma duodecim.

Solve in
Aqua font. destil.
Gramma duodecim.

Filtra per chartam.

The quantity required for each injection is, Dr. Lorigiola states, from four to twenty drops, according to age. The learned doctor says that the operation is painless and is never followed by poisonous effects. Converted into English equivalents, Dr. Lorigiola's solution will represent *one grain and four fifths* of sulphate of strychnia and *three drachms* of water. Dr. Lorigiola, who is Chirurgo Primario del Civico Ospedale di Rovigo, will doubtless be glad to learn that

Baron von Langenbeck, the distinguished German surgeon, has obtained excellent results from a less dangerous drug than strychnine, which, however safe in the hands of so able and accomplished a surgeon as Dr. Lorigiola, cannot be regarded as quite devoid of risk when injected subcutaneously. Baron von Langenbeck, in a communication on the employment of ergotine in surgery ('Berlin Klin. Wochenschrift,' No. 52, 1873), states that he has successfully used ergotine in cases of procidentia, invagination, or intussusception of the rectum. The solution is injected into the peri-rectal tissue.

This method of treatment certainly deserves attention in a disorder so troublesome as prolapsus ani.

6. Two communications on the relation of croup and diphtheria to each other appeared a little while since in the same numbers of the 'Lancet' from, respectively, the two able physicians above named. The importance of these papers cannot be over-estimated. They embody the latest thoughts, reflections, researches, and observations of two singularly competent scientific physicians, and as such are entitled to our careful and respectful consideration, not alone because of the eminence and competence of the physicians in question, but also on account of the importance of the views propounded and conclusions set forth by them. The chief and most weighty conclusion advanced is nothing less than that croup and diphtheria are identical. This clinically most important statement is one which, when duly confirmed, as it doubtless will be by many other observers, will form a definite advance in practical medicine, inasmuch as it simplifies our knowledge of disease, and fortifies us in the treatment of it. It should not be forgotten that the same opinion was long since advanced by Bretonneau, of Tours, and upheld in this country by Dr. Semple, though not generally accepted.

It is obvious that if these views prevail the belief that has hitherto obtained that croup is essentially a disease peculiar to children must be materially modified. Diphtheria, as every one knows, is not confined to childhood, though many of its peculiar manifestations may be remarkably pronounced in the earlier years of life, *e. g.* difficulties of respiration, owing to the relative smallness of the larynx and the marked tendency to spasm seen in children. Of course, it must be clearly understood that laryngismus stridulus, "spasmodic" or "false croup," as it is called—a disease which is quite peculiar to infants—is not in question. What has hitherto been known as "true croup," croup attended by exudation, is now declared to be really due to diphtheria, abundant evidence being adduced in support of this view. Sir W. Jenner says that diphtheria commencing in the larynx is exceedingly rare, that is, that it is very rare to get the exudation first in the larynx. Usually it commences in the pharynx and spreads downwards into the larynx. It is, therefore, important that in all cases of "croup" the pharynx should be at once examined.

Catarrhal laryngitis with spasm is readily cured by emetics, not so, of course, with those cases of "croup" which are due, as Sir

W. Jenner says all cases of "true croup" are, to diphtheria. Later years have satisfied him that in cases which presented all the characters of "true croup," which are sporadic, spread to no other person in the house, come on apparently from exposure to cold and damp, are attended by albumen in the urine, a symptom which formerly he thought only present in diphtheric cases. The anatomical characters of "true croup" and diphtheria being undistinguishable by the best pathological anatomists, the clinical features being the same, Sir W. Jenner practically pronounces for the identity of the two affections in the following words:—"So my opinion has undergone some modification, and I am inclined now to the belief that there is no such disease as idiopathic, simple, membranous inflammation of the larynx. I say I am inclined to this belief. I am not sure that it is true; but as I formerly thought that the weight of evidence was in favour of their non-identity, I am now inclined, from my further experience, to think that the two diseases are really identical, that the so-called croup is really diphtheria."

Dr. George Johnson is more emphatic. He states in that part of his excellent contribution which deals with the pathology of diphtheria: "I wish to express emphatically my entire concurrence in the conclusion long since arrived at by Bretonneau, Trousseau, and all the leading French pathologists, that all cases of so-called croup which are associated with the formation of false membranes in the air-passages are essentially diphtheritic; and, on the other hand, that what we in this country call inflammatory croup, or catarrhal laryngitis, never results in the formation of false membrane."

To those practising amongst children the foregoing statements must be of singular interest and importance; for it is obvious that enlightenment on the pathology of the affections should bring improvement in their therapeutical management. Probably the old heroic treatment for "croup"—bleeding, calomel, tartar emetic, and other "depressants"—has long been on the wane among the best practitioners, and it is to be hoped that it has now received its death-blow, and that a more generous and supporting medication will supplant it.

7. Professor Dohrn, of Marburg, relates a case of this rare description. The mother was 41 years old, and this was her second pregnancy. She entered Dr. Dohrn's clinic with the characteristic symptoms of well-marked purpura, which appeared at the beginning of the eighth month of pregnancy, but which under suitable treatment had almost disappeared at the time of delivery at full term. The labour was normal, and there was no hæmorrhage of importance.

The child at birth showed purpuric spots resembling those of the mother, but no other abnormity. The spots were rapidly absorbed, as also were some which appeared after birth.

8. Dr. Pfannkuch writing on this subject says it is now well proved that the cord can be spontaneously lacerated during labour. This may be produced in two ways—either by general stretching or by recoil. The first can only rarely happen, for since the fundus of the uterus follows the advancing child, the cord, in spite of its

frequent coiling, remains sufficient, and the elasticity of the tissues prevents laceration by too excessive stretching. The second occurs more frequently. In this way the cord has been lacerated whilst the woman was lying quietly on her back, the child being expelled far from the genitals. The most common cause is the fall of the child through labour coming on when the woman is in an unusual position, as when standing. The author has made a series of experiments to ascertain the force required to lacerate the cord, in the following way. The placenta was wrapped up in a piece of coarse linen and hung freely; the navel string was passed through a hole in the middle of the cloth, and to its end a bag was attached to receive the weights. In a first series of experiments, twelve in number, the distance fallen through was equal to the whole length of the cord; in two cases 500 grammes was sufficient to tear through the cord. But as in labour the child can never fall through the whole length of the cord, a second series of experiments was performed when the distance fallen through was half the length of the cord. Here in six cases the cord was torn through by a weight of 700—1000 grammes; in the other cases the injury was so severe that a slight increase of the weight would have been sufficient to lacerate the cord. The anatomical structure of the cord explains how it is it affords so little resistance to a sudden laceration. All the parts have one after the other to sustain the full force of the lacerating weight; first the amniotic covering in the concavity is broken through, then usually the arteries follow, then the vein, and last of all the convexity of the cord. The more suddenly the force acts, the nearer the laceration usually is to the point of application of the weight. The author concludes by saying that in all cases of labour in which the whole weight of the expelled child can act upon the cord there is not only the greatest probability, but it is almost certain the cord will be lacerated.

9. Dr. Faloy defines acute pemphigus of the newly born as follows: An exanthematic, cutaneous disease, nearly always apyretic and without gravity, often epidemic and sometimes contagious, rarely mortal, characterised by a discrete or general eruption of vesicles which develop simultaneously or successively, filled with a liquid at first limpid, then opaque, and giving rise to an epidermic exfoliation with impetiginous crusts and superficial ulcerations, terminating by macula which gradually disappear ultimately, leaving but traces of the phlegmasia or simple solution of the continuity of the tegumentary surface.

After reviewing the different opinions of authors on the causes of pemphigus Dr. Faloy is led by the observations he has made to the opinion that neither divers cachexies, nor diatheses, nor syphilis are the ordinary causes of pemphigus neonatorum. He contends, on the contrary, that most of the children attacked by this eruption present no symptom of general debility. The author admits that pemphigus is an exanthem which may coincide with the general state of the economy, but, as M. Hardy has said, having always the character of being an accidental and not a necessary manifestation of a per-

manent constitutional condition. The state of debility sometimes observed in these children may be the result of the exanthem and not the cause of it. Pemphigus presents itself in two very different forms: (1.) Acute benign pemphigus. (2.) Acute malignant pemphigus. In the first case it may be febrile or apyretic, simultaneous or successive, partial or general, syphilitic or non-syphilitic, pruriginous or impetiginous. In the second form pemphigus is always febrile; it may be solitary, forming but one vast bleb over the whole surface of the body, or it may be constituted by two, three, or four huge blebs, distinct at first but tending constantly to unite.

The prognosis is very different in the two kinds; recovery is the rule in the first, while death is always the consequence of the second. Dr. Faloy does not think syphilis is very often a cause of pemphigus; he attributes it rather to excessive heat.

10. Dr. Cupples records the successful removal of a dermoid cyst of the ovary from a child seven and a half years old. The abdominal enlargement had been first observed about four months before the date of operation. One noteworthy point in the case is the occurrence of an accident by which the abdomen was injured by the fall of a heavy piece of timber three years previously.

Dr. Cupples mentions that he secured the pedicle "by a single carbolized catgut ligature, divided and returned within the cavity." The operator is to be congratulated on his success; nevertheless, a caution is necessary against trusting too much to catgut ligatures, which certainly have a greater tendency, however skilfully applied, to become unfastened than have ordinary ligatures.

Carbolized catgut is obviously a very desirable material for the *ligature perdu;* but the results obtained by ligaturing the pedicle and returning it within the abdominal cavity are unquestionably inferior to those obtained by securing the pedicle outside the abdominal wound by an efficient clamp. The unparalleled experience of Mr. Spencer Wells warrants and amply confirms this statement.

11. In a paper read by Dr. de Sinéty before the Société de Biologie of Paris, the author avers that the investigations prove the occurrence of lactation in newly born infants; nay more, he asserts that the fœtal breasts may contain colostrum. He states that, as in the adult, colostrum is first found, and that subsequently the secretion becomes like that of the suckling mother. The sex of the infant makes no difference. He bases his statements on chemical and anatomical investigations.

12. Dr. Wilhite reports fourteen cases of trismus nascentium, in support of Dr. Marion Sims' theory that the disease is due to mechanical pressure on the medulla and nerves by a displacement inwards of the occipital bone. This may occur in protracted labours and in those cases where the fœtal cranial bones are too fully ossified. He describes two forms—the acute, where death takes place within two or three days, and the chronic, in which the child gradually wastes away. The first symptom in both forms is inability to suck, and this, he says, is pathognomonic of the disease. The treatment should consist in keeping the child lying on the side, not on the back, so that there is no pressure upon the occipital bone.

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O Correio Medico de Lisboa. (Fortnightly.)

NOTICE TO READERS.

The Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him, as also Inaugural Lectures, Dissertations, or Theses, Medical and Scientific Addresses, &c.

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